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A STUDY OF CONDITIONED RESPIRATORY CHANGES [1]

BY C. R. GARVEY

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Respiratory changes following strong or emotional stimuli have been studied by Benussi (1), Blatz (2), Caster (3), Sears (16), and Ray (14), and such respiratory disturbances have been conditioned to signal stimuli by Scott (15) for the purpose of studying post-hypnotic amnesia, and by Lashley and Watson (17). In the present study, certain changes in the depth and regularity of breathing which occur as reactions to faradic stimulation were conditioned to the sound of a buzzer and to the operation of a finger vibrator. The purpose of the investigation was to study some of the more basic phenomena of conditioning, such as summation of conditioned responses to compound stimuli, differentiation of a compound stimulus from its components, and the effect of the temporal interval between test stimuli.

The subjects were 38 college boys, 6 used in the control experiment on negative adaptation, 32 in the preliminary

[1] This study was made in the research unit of Professor Clark L. Hull and under his direction. The writer is grateful to Dr. Hull for many suggestions and criticisms. He also wishes to thank Mrs. Helen G. Garvey for critically reading the manuscript. This paper was read in part before the American Psychological Association at Cornell University, Sept. 10, 1932. For studies of respiratory reactions in animals see Upton, *Amer. J. Psych.*, 1929, 41, 412–421; Wever, *J. Comp. Psych.*, 1930, 10, 221–233; Horton, *J. Comp. Psych.*, 1933, 15, 59–74.

conditioning, and 8 of the latter in the main experimental group. They were paid by the hour. The experiments were performed between April 15 and July 1, 1931.

Apparatus

The apparatus consisted essentially of 7 parts. (1) Respiration was recorded kymographically by a pneumograph similar to the Verdin except that the tambours were larger and were set at right angles to the tension on them; and by a Krueger (10) non-arcing tambour stylus. (2) The signal stimuli were produced by (*a*) a buzzer and (*b*) a finger vibrator consisting of a doorbell mechanism with the chime removed and the tongue increased to a uniform quarter-inch diameter by wrapping with wire. This vibrating part fitted between the middle and ring fingers of the subject's left hand as it lay on the arm of the chair. (3) The shock was delivered through a pair of silver thimbles soldered to the end of an insulated cable. A small sponge wet with a saturated NaCl solution was fitted into each thimble, the thimbles were put on the index and middle fingers of the right hand, and the cable lashed to the wrist with tape. The thimbles were insulated so that they could not short the shock circuit by touching each other. The primary of the Harvard shock coil was connected with an A.C. source through a transformer, and was operated continuously, only the secondary being opened and closed. This did away with the sharp and variable inductance phenomena at the beginning and end, and delivered about 6 impulses of uniform intensity. The primary was 10 volts, and the position of the coil varied from 6 to 0, depending on the subject, so as to produce a clearly perceptible reaction or clinical picture of response to shock, including some such signs as flinching, grimacing, or catching the breath. The shocks were described by the subjects as strong or severe, and the reactions would be classified as emotional in the terminology of previous writers. (4) All-or-none records were made of finger reactions to the shock. The thimbled fingers rested on a pair of thin flat springs fitted to the chair arm, so that the springs yielded to any movement of the fingers. The action of the springs was transmitted pneumatically through an adjustable systematic leak to a highly sensitive switch which responded to any perceptible movement of either finger by closing a circuit through a signal marker. (5) A time-line was obtained with a 5-sec. marker controlled by an A. C. clock. (6) A masking sound was produced by phones on the subject's ears. The phones were operated by 8 volts interrupted 120 times per second by a relay driven by A. C. The noise was sufficient to prevent the subject's hearing any of the other apparatus except the buzzer, which was very loud and raucous and broke sharply through the sound screen. (7) The stimuli were timed by a fast pendulum. The buzzer, vibrator, and shock were actuated by small flick switches consisting of two thin strips of shim brass which were brushed against each other by the pendulum as it fell. On account of the brief contact in this kind of switch, a 3-pole, double-circuit relay was inserted in series with each, wired so as to hold the circuit closed until the primary of the relay was broken by the pendulum at the end of its swing. The flick switches were set in the arc of the pendulum, one to operate the buzzer (or vibrator, or both, depending on the setting of manual switches) for .250 sec., and one to operate the shock for .050 sec. before the circuits were broken. They were all broken by the same switch. There were four manual switches, one of which set up the circuit for the buzzer, one for the vibrator, one for the shock; the fourth controlled the magnetic circuit which held and released the pendulum. It was found that the time during which the signal circuit was closed varied from .248 to .252 sec., and the shock circuit varied from .048 to .052 sec.

PROCEDURE

The subject was brought into the laboratory and (unless he was a control subject for the study of adaptation) was asked if he would mind receiving shocks if they should become necessary during the experiment. He was allowed to feel a minimal shock from the bare poles of the inductorium. The pneumograph was adjusted approximately over the eighth rib. The subject was blindfolded and told to relax and assume a comfortable posture. This is rather important since it is hard for most subjects to sit still 30 to 50 minutes, and any shift in posture during the experiment is likely to disturb the breathing record.

The phones were then placed over the ears. The sound screen was started with small voltage and the subject was asked if he heard a clicking sound. When he answered, the voltage was stepped up and he was asked "Now is it louder?" The answer was always, 'Yes,' whereupon all the resistance was eliminated and the question repeated. This time it was never answered.

In the preliminary series, the subjects were given several signal (buzzer or vibrator) stimuli, then several such signals reinforced by shock, the strength of the shock being increased until it produced an observable reaction. Several test signals were then given, sometimes followed by more reinforcement and additional tests. The number of stimuli was varied from subject to subject for the purpose of exploration. Those who had been subjected to the buzzer on their first day were tried with the vibrator on their second day and vice versa.

For the control group on adaptation, the employment clerk was asked to say nothing to the students about shocks unless they inquired, in which case she was to say that there were no shocks in this experiment. No students were employed who had previously been subjects in the conditioned reflex laboratories. When the subject came to the laboratory he was told that he was to be used as a control, the idea being to establish a norm on the physiology of quiet rest when no stimuli or shocks were given. Only the buzzer or the vibrator was used with these subjects, no shock was given, and the subjects were not used in any other part of the study.

From the 32 subjects used in the preliminary series, 8 were selected for further study on the basis of their adaptability to the experimental situation,[2] ability to sit still, availability, and conditioning to the signals already used. The procedure with these subjects is described in the appropriate places, *e.g.*, in the section on differentiation.

In all the experiments the stimuli were separated by intervals of 45, 60, 75, and 90 sec. the different intervals were distributed in a chance order which had been determined beforehand by tossing coins, and which was the same for all subjects in a given group, except in the preliminary series. The experimenter counted the clicks of the time marker till he arrived at the appropriate number, then glanced at the respiration marker on the record, and released the pendulum by pressing the key *when the marker paused at the end of the next expiration* (see Fig. 1). He then checked this stimulation on his program, observed the next interval to be used and resumed counting. A section of record is shown in Fig. 1, which demonstrates the speed of the drum, the size of the respiratory tracing, and the arrangement of the markers. The lower points

[2] It is well to avoid subjects who produce highly variable curves, those of the Cheyne-Stokes type, or of Nielsen and Roth's Type G: normal inspiration line interrupted by frequent and deep inspirations (sighs) (11).

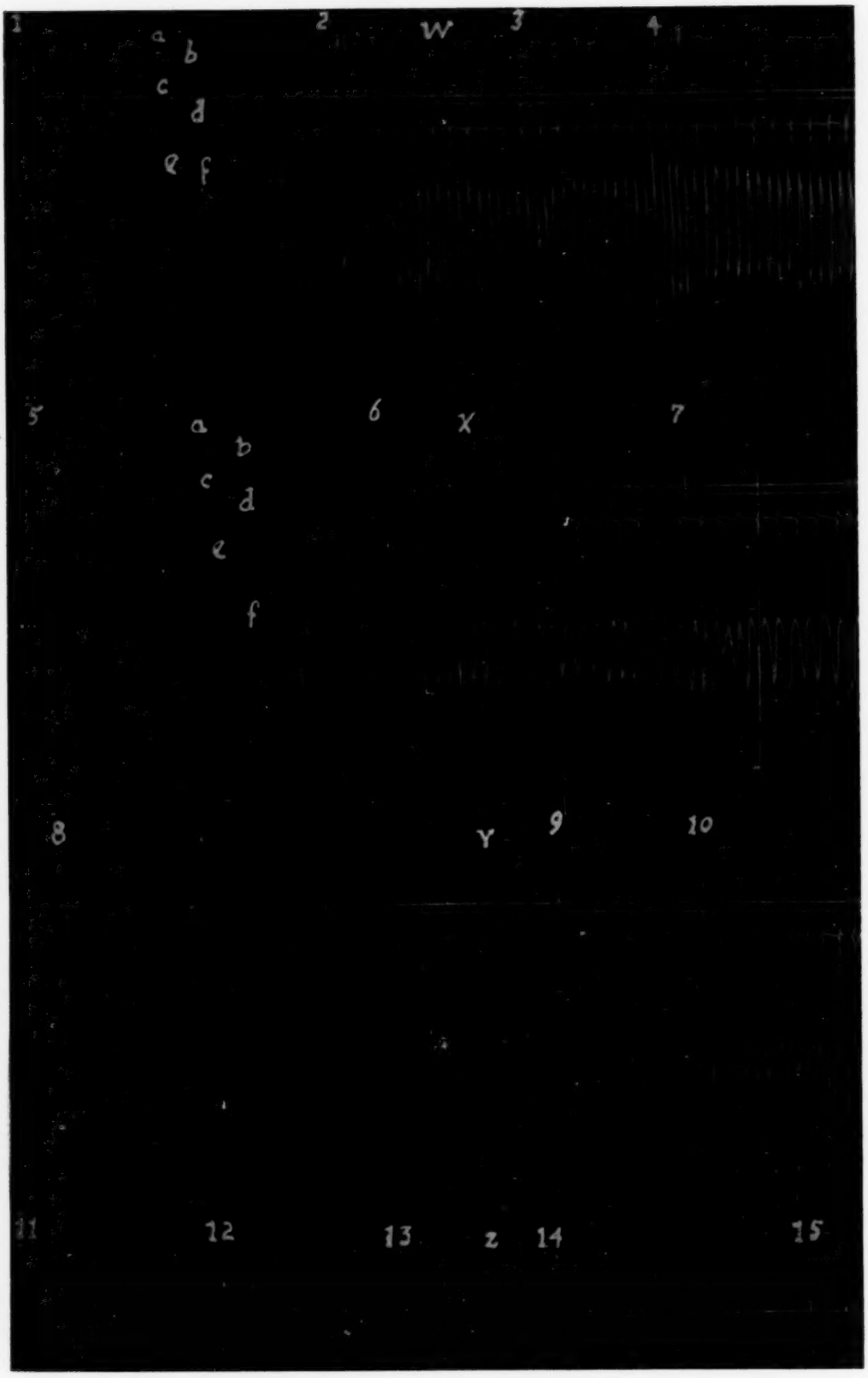

FIG. 1. Photograph of sample record. Actual size.[3]

on the curve represent the ends of inspirations. The records were measured for *irregularity* of inspiration and for *total oscillation*. In measuring the irregularity of breathing, the oscillometer pointer was moved from one of the lower points of the tracing to the next, etc., starting with the last one before the presentation of the stimulus and going to the right for the post-stimulus period, or to the left for the pre-stimulus period. In measuring the total oscillation of the tracing, or the amount of breathing, the pointer was moved from the same starting point to the next upper point, from there to the next lower point, then to the next upper point, etc. The methods described in detail by Sears (16, 10, 12 ff.) were followed with minor exceptions. In measuring the irregularity of the inspiration depth, we used 5 or 10 breaths each in the pre- and post-stimulus periods, whereas he used 20 sec. in each. This makes it unnecessary for us to divide the pre- and post-stimulus values by N to make them comparable, so that our measure of response is post-stimulus value minus pre-stimulus value, whereas his is post-stimulus value over N minus pre-stimulus value over N'. Here N is the same as his $N - 1$, since we count the starting point as zero, hence he would count our 5 inspirations as 6, etc. By irregularity we mean *serial* variability, which Sears measured and called *variability*, a term which we abandon in order to avoid confusion with statistical variability.

Unconditioned Responses to the Signal Stimuli

In order to ascertain whether the presentation of signals which had not yet been conditioned caused any change in breathing, data relative to this point were extracted from the records of 30 subjects in the preliminary conditioning group. These include subjects who later showed conditioning and those who did not. There were from 1 to 12 presentations of the buzzer to each of 15 subjects, and from 5 to 11 presentations of the vibrator to each of the other 15 before the respective subjects had received any shocks in the experiment. The data were analyzed in three ways. First, the responses were measured by the difference between the respiratory irregularity of 10 pre-stimulus and 10 post-stimulus inspirations. Second, the same method was used with 5 inspirations instead of 10. Third, the responses were measured by the difference in total oscillation of the respiratory tracing during a 20-second

[3] This is a portion of kymograph record showing a section of tracing from each of 4 subjects, W, X, Y, and Z. The markers record (*a*) finger movement, (*b*) shock, (*c*) vibrator, (*d*) buzzer, (*e*) 5-sec. intervals, and (*f*) respiration. The stimulations are numbered. Buzzer and shock reinforcements, with finger reactions and increase in amount and irregularity of breathing are shown for example at (2) and (3) with subject W, and (8) and (10) with subject Y. Large respiratory changes and definite finger reactions to the buzzer and vibrator without shock are shown at (6) with subject X and (9) with subject Y. Starting points for the oscillometer readings are indicated by slanting check marks on the record of subject W. The long vertical lines on the record of subject X mark the beginning and end of the 20-sec. pre- and post-stimulus periods.

pre-stimulus and a 20-second post-stimulus period. According to the first method, 94 responses to the buzzer averaged 6.51 oscillometer units with a sigma of 18.82, yielding a critical ratio of the mean over the sigma of the mean of 3.36. Likewise 135 responses to the vibrator gave an average of 4.57, a sigma of 19.73, and a critical ratio of 2.69. These figures summarized according to the author's method (**6**, equation 5 or 6) yield a mean for both types of signal of 5.37, a sigma of 19.39, and consequently a critical ratio of 5.37.

According to the second method, the 94 responses to the buzzer gave an average of 3.59, a sigma of 13.14, and a critical ratio of 2.66; the 135 responses to the vibrator gave an average of 1.39, a sigma of 10.71, and a critical ratio of 1.51; and all responses for both signals averaged together yielded a mean of 2.29, a sigma of 11.82, and a critical ratio of 2.94.

According to the third method, data are available on only 17 subjects (7 with the buzzer and 10 with the vibrator), because of an interference with the timing system in another part of the laboratory. A summary of this material shows a total of 118 responses with a mean of 8.90, a sigma of 22.85, and a critical ratio of 4.24. There is therefore no doubt that there is a small though significant response to the buzzer and vibrator signals before either of these has been presented with a shock. This is either an unconditioned response or a result of very generalized conditioning by previous association of buzzes and vibrations with electric shocks. It was not due, apparently, to expectation of shocks, for the magnitudes of the responses in the control group, described in the section on adaptation, compare favorably with corresponding values in the present series. The mean of 5.37 by the first method above should be compared with 4.0 in the control series, and 2.29 by the second method with 2.7 in the control.

Adaptation

When an unconditioned signal stimulus such as the action of a buzzer or finger vibrator is presented to the subject repeatedly without a conditioning stimulus, the effect upon the subject's breathing shown in the preceding section might be expected to diminish as the repetition continues. This nega-

tive adaptation to the signal does occur, as is shown graphically in Fig. 2. Each point on the curve is an average of 5 values on each of 6 subjects, except that for the last point, 2 subjects furnished only 3 values each. These are control subjects not used in any other part of the experiment. The individual values are obtained by subtracting the irregularity of the 5 breaths preceding the signal from the irregularity of the following 5.[4] This factor was also studied by using 10 breaths instead of 5, and by measuring the total oscillation of the respiratory tracing during 20 seconds preceding and following the signal. The curves produced by these methods are not as smooth as the one shown, but are similar to it in that their main characteristic is a general decline, extending to zero and below. This extension of the adaptation process below zero, *i.e.*, to the point where the stimulus causes a decrease rather than an increase in the respiratory irregularity,

TABLE 1

ADAPTATION

The diminishing effect of the stimuli is shown in the column headed 'mean of responses.' The 'response' is the difference between the pre- and post-stimulus respiratory irregularity, in oscillometer units.

Responses in the Series	Mean of Responses on 6 Subjects	N	Sigma	Range	Pre-stimulus Mean
1st 5	2.7	30	9.46	28 to −18	7.7
2d 5	0.9	30	10.84	20 to −31	10.0
3d 5	1.7	30	7.54	17 to −18	10.9
4th 5	1.0	30	11.31	28 to −21	10.0
5th 5	0.0	30	11.12	26 to −21	11.8
6th 5	−2.6	26	17.77	36 to −65	12.1

[4] The great amount of variation encountered in work upon the respiratory responses to simple stimuli should be considered by anyone who contemplates such work. For this reason the range of variation as well as the mean and standard deviation is given in Table 1, along with the pre-stimulus mean irregularity. Note the enormous overlapping in ranges. The variability is such that the only difference between points on the curve which the writer is willing to consider at all statistically significant is the difference between the first and last points, which is 1.7 times its own sigma. The increase in pre-stimulus values is rather general, indicating an increase in biological variability as the experiment progresses. The largest difference in the column is, however, only 1.1 times the sigma of the difference. Respiratory responses are better suited to study of electric shocks and of intense pain such as that produced by the Sears algesimeter (16, 5 ff.). For another example of the variability of respiratory responses to non-painful stimuli see Scott (15). For another example of the clear effect of strong stimuli, such as electric shocks and gunshot sounds, see Caster (3, 137, 144).

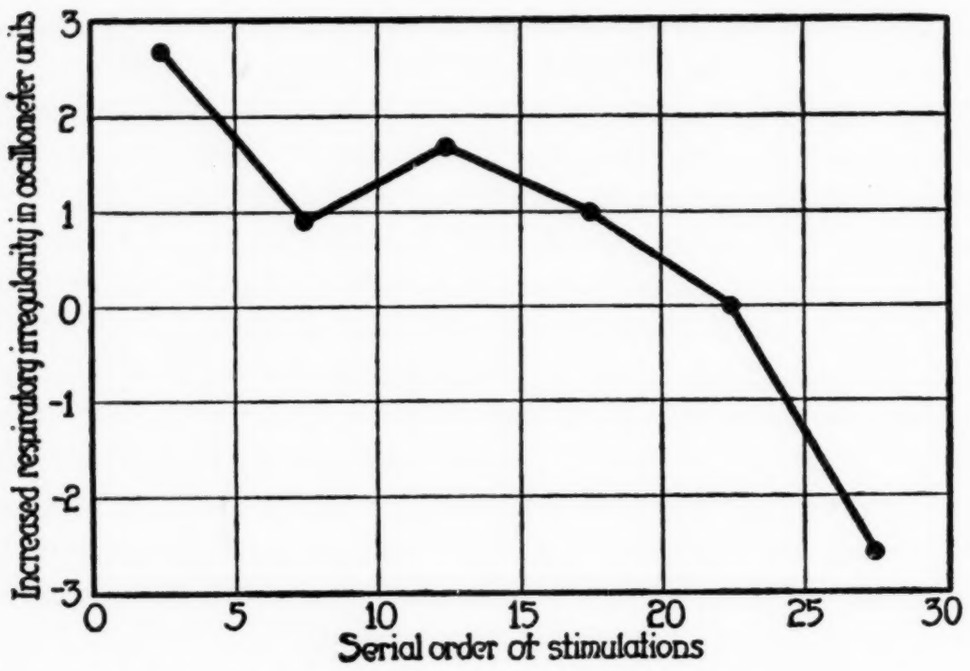

FIG. 2. Adaptation: The diminishing effect of repeated signals (buzzer or vibrator).

raises the question as to whether adaptation is really a dropping out of response—a disregard for the stimulus—as is commonly supposed, or whether the process is really one of inhibition. A third and more likely alternative is that adaptation is a combination of both processes; inhibition of the response and a growing ineffectiveness of the stimulus, the inhibition being only a temporary phenomenon. This could be tested by continuing the repetitions indefinitely to see if the curve returns to zero and remains there. The view of adaptation as mere decreased effectiveness of the stimulus may derive from the fact that the ordinary experiment on adaptation utilizes responses which can decrease only to zero, and methods of recording and analysis which can reveal decreases only to that point. The problem of the nature of adaptation should be studied further by experiments upon other processes which admit of negative values, as well as upon the one under consideration.

CONDITIONED RESPONSES

Evidences of conditioning are revealed by comparison of the irregularity of 10 breaths before and 10 after stimulation

in 19 of 32 subjects, by 5 breaths before and 5 after stimulation in 22, by both criteria in 17, and by one or the other in 24 of the 32.

If we consider the possibility of conditioning's being manifest in a decrease rather than an increase in the irregularity of the breathing, then we find evidences of this kind in 15 of the 32 subjects by either criterion, 13 by the former, 10 by the latter, and 8 by both. These decreases in irregularity are small in most subjects in which they occur, however, as compared with the increases in the other subjects, and are therefore not considered strong evidence of conditioning. The normally expected response to a painful or an emotional stimulus is, then, an increase in irregularity of the breathing.

The evidence of conditioning in the 8 subjects selected for the main experimental group is as follows. The mean response (mean of the 8 individual averages) to the first signal given prior to reinforcement was 3.33 oscillometer units (increased irregularity of 10 post-stimulus breaths). The sigma of this mean is 1.32, making the critical ratio 2.52 and the mean significantly greater than zero. After a period of reinforcement the mean response to the signal was 11.04. The sigma of the difference between these means, the one before and the one after conditioning, is 3.81, taking into account the correlation of .37, and the critical ratio is 2.02, which indicates that the difference due to conditioning is probably significant. Since adaptation of the original responses takes place rapidly unless the signal is reinforced, it is suggested that the value used as a measure of conditioning should not be the difference between pre- and post-reinforcement values, but the post-reinforcement value itself. The mean (11.04) of the conditioned responses divided by its sigma (4.10) gives a critical ratio of 2.69.

Measured by the increased irregularity of 5 post-stimulus breaths, the conditioned responses average 7.98 units larger than pre-conditioning responses, and this difference is 3.68 times its own sigma. The conditioned response values average 10.98 with a critical ratio of 4.16.

The data on total oscillations are incomplete and cannot be presented here.

The conditioned responses to the second signal to which the subject was conditioned, tested on the second day of experimentation, show similar results. Measured by the increased irregularity of 10 inspirations, the mean conditioned response is 5.48, an increase of 2.90 over the mean values before reinforcement. The increase is 4.03 times its own sigma, and the critical ratio for the mean conditioned response itself is 4.25. Measured by the increased irregularity of 5 inspirations the mean conditioned response is 7.58 with a critical ratio of 5.41. This mean is greater than the corresponding value before reinforcement by 5.66 oscillometer units, a difference which is 5.96 times the sigma of the difference.

The increased total oscillation of the respiratory tracing reveals a mean difference between conditioned and pre-conditioning response of 2.54 units, which is not statistically significant, the critical ratio being only .53. The mean conditioned response itself is 13.43, however, with a critical ratio of 5.71. From the foregoing paragraphs it may be concluded that this group of 8 subjects have now been conditioned to make respiratory responses to either of the two signals. These facts are derived from the data which are summarized in Tables 2 and 3 and illustrated in Fig. 3.

Conditioned finger reactions were established in fewer subjects than were the respiratory changes. Also, they seemed to be less stable, although they appeared later as a result of summation, even in subjects who had shown no conditioned finger reactions in the preliminary series. Of 31 subjects given 6 to 23 reinforcements each, 8 subjects made finger reactions to the signals after but not before reinforcement (conditioned responses), 3 made more responses after than before (probably conditioned), 1 made fewer responses after than before, 1 made none after reinforcement, while 18 made none before or after. Four of the latter made no finger reaction even when the shock was delivered. Of those who did react, only one-third moved the finger perceptibly *every* time they were shocked. Those who were definitely conditioned usually showed only 1 or 2 conditioned responses

TABLE 2

THE INCREASE IN IRREGULARITY OF A NUMBER OF POST-STIMULUS INSPIRATIONS OVER AN EQUAL NUMBER OF PRE-STIMULUS INSPIRATIONS IN RESPONSE TO A SIGNAL BEFORE, DURING, AND AFTER REINFORCEMENT WITH ELECTRIC SHOCKS

B stands for the buzzer, V for the vibrator, and E for the electric shock. These are the subjects who were not used in the main experimental group. The data on those subjects are isolated in Table 3.

Subject	Stimulus	N	Mean Response, 10 Inspirations	Mean Response, 5 Inspirations
2	B	4	7.3	7.0
	BE	10	7.0	3.9
	B	6	1.8	2.8
3	B	1	3.0	0.0
	BE	13	8.4	6.3
	B	3	1.0	0.3
5	B	5	9.0	8.2
	BE	9	16.3	14.8
	B	8	11.3	7.4
6	B	5	15.6	9.2
	BE	7	11.4	6.9
	B	6	13.3	5.3
7	B	3	−3.0	−1.3
	BE	8	37.3	28.1
	B	4	37.0	24.0
8	B	7	11.1	4.6
	BE	14	13.6	7.5
	B	7	16.4	14.3
11	B	10	0.9	0.1
	BE	19	21.4	16.3
	B	8	12.5	4.1
12	B	7	9.3	4.7
	BE	15	4.4	3.3
	B	10	6.2	1.0
13	B	7	1.3	−0.4
	BE	6	27.2	26.8
	B	12	26.8	12.0
14	B	5	−1.8	−3.4
	BE	10	5.6	6.6
	B	7	12.7	6.4
15	B	7	15.3	0.4
	BE	12	23.0	14.2
	B	10	7.1	0.6

TABLE 2 (*continued*)

Subject	Stimulus	N	Mean Response, 10 Inspirations	Mean Response, 5 Inspirations
16	B	8	6.0	3.5
	BE	10	23.7	21.9
	B	10	16.5	12.9
17	V	7	4.1	2.7
	VE	11	−0.4	−1.8
	V	10	3.1	0.7
18	V	9	−1.9	−1.7
	VE	9	7.1	6.2
	V	2	38.0	7.5
20	V	11	8.8	3.6
	VE	7	8.6	6.6
	V	6	0.2	−2.0
22	V	10	19.3	7.9
	VE	6	9.3	9.2
	V	2	5.5	2.5
24	V	11	12.8	3.7
	VE	6	12.8	1.3
	V	6	3.6	4.3
25	V	11	8.8	6.0
	VE	9	10.6	12.3
	V	5	−3.4	0.6
27	V	10	10.3	2.1
	VE	10	26.4	16.4
	V	7	21.6	11.1
28	V	5	0.2	1.6
	VE	8	3.4	5.5
	V	7	0.9	0.3
29	V	10	1.0	−4.0
	VE	7	−4.7	1.7
	V	8	9.4	3.5
30	V	8	3.3	4.9
	VE	8	7.9	6.5
	V	6	2.7	2.2
31	V	10	−2.4	2.0
	VE	8	6.5	2.9
	V	10	9.5	0.2
32	V	10	−0.3	−3.0
	VE	8	13.3	9.6
	V	7	6.4	6.6

and then extinction until reinforced again. Only one subject made 4 conditioned responses in succession.

Transfer

Do the effects of conditioning to one signal transfer to another signal which is later presented in the same laboratory setting? A number of subjects were tested for reaction to one signal, then conditioned to it with shocks, and later tested for reaction to another signal. In order to control any effects of the differences between the two signals, half the subjects were run with the order of the signals reversed, and the data on the two signals were analyzed separately as well as together.

TABLE 3

Main Experimental Group. Information as in Table 2

For any subject the signals were conditioned on separate days, in the order in which they are given in the table.

Subject	Stimulus	N	Mean Response, 10 Inspirations	Sigma	Mean Response, 5 Inspirations	Sigma
1	B	1	−2.0	0.00	0.0	0.00
	BE	11	9.8	8.78	7.5	5.93
	B	7	6.1	5.16	5.1	5.22
	V	12	−0.1	5.01	0.7	1.00
	VE	15	30.9	11.20	28.1	9.18
	V	14	0.6	8.31	0.9	4.34
4	B	2	0.5	0.50	0.0	1.00
	BE	23	3.4	8.26	2.7	6.39
	B	10	5.6	7.98	4.9	8.60
	V	10	4.8	9.60	1.9	4.48
	VE	14	6.4	10.72	6.6	7.23
	V	16	7.8	11.55	6.8	10.10
9	B	12	11.1	28.39	8.7	24.92
	BE	14	20.0	28.12	16.6	16.41
	B	7	12.6	36.02	14.6	24.20
	V	20	6.3	24.61	7.3	15.79
	VE	12	16.5	37.43	16.3	23.50
	V	22	6.5	21.03	13.5	13.87
10	B	11	3.7	22.87	4.1	19.25
	BE	8	31.5	18.41	34.5	21.59
	B	16	15.6	34.54	11.8	21.55
	V	15	7.0	17.44	5.7	14.91
	VE	7	23.0	11.00	21.6	7.96
	V	18	13.0	23.06	11.4	14.49

TABLE 3 (*continued*)

Subject	Stimulus	N	Mean Response, 10 Inspirations	Sigma	Mean Response, 5 Inspirations	Sigma
19	V	7	2.4	11.67	0.1	8.58
	VE	14	20.1	23.85	16.5	18.40
	V	8	−1.9	13.22	3.3	5.13
	B	10	0.6	3.35	−0.1	1.81
	BE	10	30.2	15.71	23.3	15.62
	B	20	4.6	8.84	4.8	9.91
21	V	7	4.3	26.06	3.9	9.58
	VE	12	24.7	21.20	12.4	18.46
	V	3	37.3	30.96	27.7	23.66
	B	11	1.1	8.76	1.0	2.70
	BE	6	15.7	25.14	8.5	13.56
	B	15	6.5	19.94	11.6	14.92
23	V	10	0.7	13.89	1.6	8.01
	VE	8	12.9	38.89	21.0	28.94
	V	8	−0.6	20.00	6.9	17.95
	B	11	0.9	13.28	−1.4	7.79
	BE	10	25.6	17.51	19.9	12.82
	B	22	3.9	13.37	5.5	8.53
26	V	10	5.9	19.48	5.6	8.51
	VE	8	26.3	12.71	19.6	20.22
	V	8	13.6	24.82	13.5	20.96
	B	10	0.0	8.97	0.2	8.30
	BE	8	37.5	15.70	26.4	7.39
	B	15	0.9	10.59	6.1	7.42

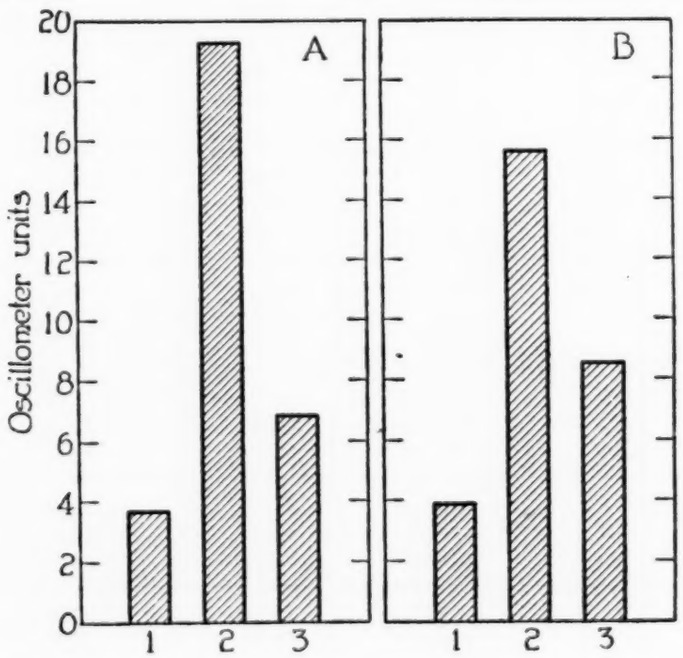

FIG. 3. Magnitude of response (1) before, (2) during, and (3) after reinforcement in case of (*A*) 8 selected subjects and (*B*) all 32 (unselected) subjects. Data summarized from Tables 2 and 3.

A study was made of both the irregularity and the total oscillation of the respiratory tracing. The results were inconsistent and no conclusion can be drawn from them. The question remains open, since the results were not statistically significant and their inconsistency is probably due to the insufficient numbers resulting from the incidental nature of the observations. It is also thought that negative adaptation may have reduced the slight differences which were found.

An Unusual Example

The experiment outlined in Table 4 is shown as an example which illustrates beautifully the phenomena of conditioning but is nevertheless not a typical example. In fact, its most unusual feature is that it does illustrate these phenomena so clearly. The vibrator when reinforced by the shock produces a response of 53 units. Then the vibrator alone

TABLE 4

Experiment of May 13, 1931. Subject 1

(*A Non-typical Example*)

Time	Stimulus	Increase in Amount of Breathing During 20 Seconds	Remarks
2:16 P.M.	Routine reinforcement of vibrator by shock		
2:43	Vibrator and shock	53	Reinforced
2:43¾	Vibrator	29	Not reinforced
2:45	Vibrator	10	Not reinforced
2:46	Vibrator	−24	Not reinforced
2:47	Vibrator and shock	49	Reinforced
2:48¼	Vibrator	13	Not reinforced
2:49¾	Vibrator	11	Not reinforced
3:00	Vibrator	−20	Not reinforced

produces 29. This illustrates the fact that the conditioned response is weaker than the unconditioned. On repetition the conditioned response drops from 29 to 10, then to −24, showing experimental extinction. The vibrator is now reinforced again, producing a response of 49, and on subsequent test produces 13, again showing that the conditioned response is less than the unconditioned. And again the conditioned response is extinguished.

It is hoped that the reader will remember that examples of

this kind are rare, at least in the present type of study, and that where human subjects and complicated responses are involved, no conclusions should be drawn from a single example, but only from the combined or averaged results of a number of such experiments. One frequently finds single examples of this kind in the conditioned reflex literature, and one is justified in asking if they are as typical as they are illustrative. In the present case there is no question but that the example is not typical. Furthermore, one would need only to record the series of values under the first entry in our table to see evidence of great variability within the single experiment. Also if one were to repeat the conditioned stimulus a few more times, the example would no longer illustrate experimental extinction so clearly.

'Learning' Curves

On account of the theoretical relationships of conditioning to learning it would be desirable to have representative curves of the development of conditioning comparable to learning curves. A few such curves have been presented, such as those by Hilgard on the eyelid reaction (**7**, 13 ff.), those by Wendt on the knee-jerk (**18**, 43, 50), and those by Kleitman and Crisler on the salivary reaction in the dog (**9**, 587, 595). Figure 4 shows the curves of acquisition of the conditioned change in respiration, (*A*) in our main experimental group of 8 subjects, and (*B*) in our total group of 32 subjects. Curve *A* is based on the average results of establishment of conditioning to two separate signals, buzzer and vibrator, on separate days. Curve *B* is based on an unselected group of 32 subjects, 12 of whom (including the 8 of curve *A*) were conditioned to both signals, and 20 of whom were conditioned to only one signal (10 to the vibrator and 10 to the buzzer). The number of test stimuli, as well as the number of reinforcements and the distribution of stimuli, varied from one subject to another and from one part of the experiment to another. The values of the points on the curves, based on increased irregularity of 10 post-stimulus over 10 pre-stimulus inspirations after various numbers of reinforced stimuli, and the

number of stimulations producing each value are given in Table 5.

TABLE 5

THE ACQUISITION OF THE CONDITIONED RESPONSE

No. of Reinforcements	Curve *A*		Curve *B*	
	N	Mean	N	Mean
0	159	2.96	229	5.37
1–7	51	6.22	105	8.87
8–10	86	6.60	164	8.86
11–23	73	7.26	171	8.64

Comparable curves based on measurement of 5- instead of 10-inspiration intervals were similar in form to those shown. Because of the unequal distributions of frequencies about the mid-points of the class intervals on the abscissa scale, the ordinate values of the curve are plotted at the positions of the abscissa medians rather than at the mid-points. The total number of reinforcements preceding any test stimulus includes the total number preceding any previous test. These curves are similar to those found in the literature, in spite of the different reactions utilized and the diverse conditions under which they were obtained. Whether the curves presented here

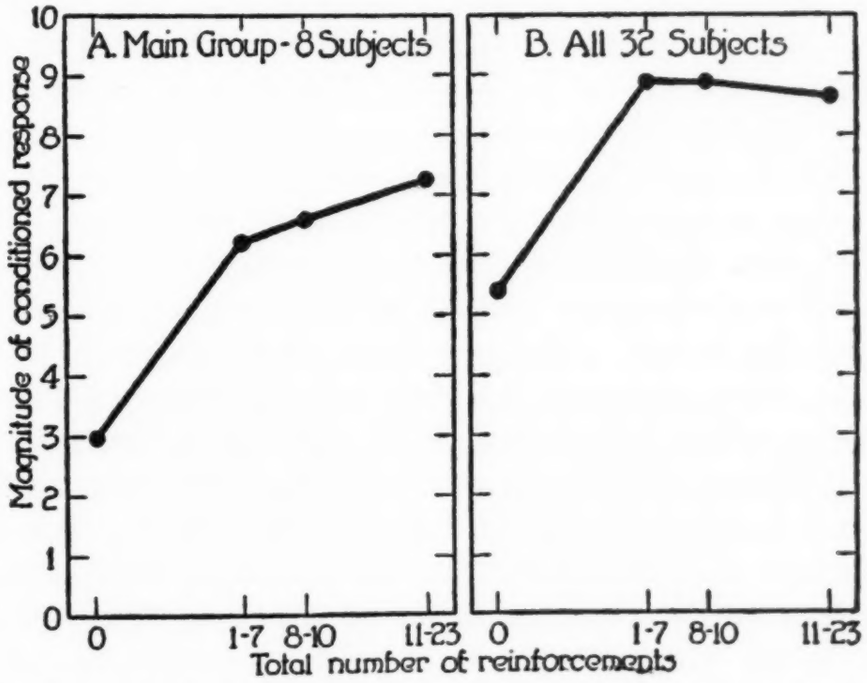

FIG. 4. Curves showing change in magnitude of conditioned response with re peated reinforcement.

are really convex or S-shaped depends on the position of additional points between 0 and 1–7.

Summation

From the general theory of conditioned reflexes and from Pavlov's results on dogs (p. 79), it should be expected that if two signal stimuli, each of which had been conditioned to the same response, were presented to the subject simultaneously the response would be of greater magnitude than would be the response to either signal alone. This would be a summation phenomenon, the conditioned response tendencies resulting from the two signals facilitating each other or combining to produce a single larger response. In order to test this assumption the 8 subjects of our main experimental group were given appropriately designed series of stimuli at the beginning of one day's session after they had been conditioned to each of the two signals on previous days. This test series consisted of one each of the two single signals and two presentations of the combined signals. In order to control the sequential relationships between the stimuli in the series the reverse-balance technique or a b b a b a a b method was used. Since there were two different single signals and one double signal, this technique yields four types of series. If we designate the buzzer signal by B, the vibrator by V, and the compound by BV, these four series are (1) B, BV, BV, V, (2) V, BV, BV, B, (3) BV, B, V, BV, (4) BV, V, B, BV. Two subjects were run on each of these series, one member of the pair having been conditioned first to the buzzer and then to the vibrator and the other conditioned first to the vibrator and then to the buzzer. The two single signal responses on each subject were averaged, as well as the two double signal responses, thus giving one single and one double value for each subject. This was done because the only expected source of correlation in the data would be the individual differences between subjects. The results were analyzed according to the three methods described in the section on the original responses to the signals. (1) According to the effect on the irregularity of 10 post-stimulus inspirations the

single signals showed a mean response of 2.00 oscillometer units for 8 subjects and a sigma of 5.74; the double signals showed a mean of 15.94 and a sigma of 12.56. The correlation between single and double values produces an r of $-.64$ calculated from the raw values. The rank difference method was used as a check and rho found to be $-.67$. The critical ratio of the difference over the sigma of the difference is 2.32, indicating a probability of 99 chances in 100 that the double signals really produce a greater response than the single ones. On account of the questionable value of a correlation on 8 subjects, the critical ratio was recalculated by the shortened formula which assumes a correlation of zero. This produced a value of 2.86, or a probability of 99.8. (2) According to the effect on 5 post-stimulus inspirations the single signal mean is 1.75, and the sigma 5.01, the double signal mean is 13.25 and the sigma 8.24, the r is .16, and the critical ratio 3.63, or if we assume the data to be uncorrelated, 3.47. (3) According to the effect on the total oscillation in a 20 second post-stimulus period of the respiratory tracing, the single signal mean is -2.50, the sigma 27.33, and the double signal mean is 24.19, the sigma 34.36. The correlation is .82 and the critical ratio 3.83, or without the correlation, 1.72.

These figures, derived by different methods on account of the variability inherent in the data, are in fundamental agreement and support the assumption of a summation phenomenon. That this is summation and not an example of what Pavlov calls overshadowing (in which the response to the compound is equal to the response to the stronger component) is not necessarily apparent from the average of the two components, since it might be conceived that one component be equal to the compound, and the other component of sufficient negative value to produce the smaller average. This is not true, however: the average of 2.00 above is not an average of 15.94 and -11.94, but actually an average of 2.38 for the vibrator and 1.63 for the buzzer. In general this condition holds as well for the other values given. We are dealing, then, not with a case of 'overshadowing' but of summation or facilitation. It might be objected that this summation could

be due to the mutual facilitation of the unconditioned stimulus values of the signals, rather than to the facilitation of their conditioned response tendencies. The answer to this is that the amount of experience that each subject had had with the single signals was enough to have produced negative adaptation to these signals, and would probably have done so [5] had not the conditioning process or reinforcement intervened. The stimulating value of these signals at this point should therefore be attributed to their association with the electric shocks on the first two days of the experiment, and the summation or facilitation effect noted should be considered that of conditioned responses. This agrees in general with the conclusion of Pavlov (**12**, 79). One difference, however, should be noted. Whereas the Russian work shows that the compound stimulus produces a response equal to or less than the sum of the responses to the single stimuli, our results indicate that the response to the compound is even larger than the sum of the single responses. To give an example: Pavlov (**12**, 79) reports, evidently from Leporsky's unpublished thesis (**12**, 416), an experiment in which three single signals had been conditioned, (*a*) which gave on one occasion 21 drops of saliva, (*b*) which gave 23 drops, and (*c*) which Pavlov fails to evaluate further than to say that all three were firmly established. Pavlov says that all three applied together produce a much greater response than any one singly, for instance all three, on the occasion mentioned above; produce 32 drops. Now the point is that even though we have no value for (*c*), it is apparent that 32 drops is less than the sum of (*a*), (*b*), and (*c*), for the sum of (*a*) and (*b*) is 44. Another example is given by Evans (**4**, 381) without an adequate reference to his source of information: Signal (*a*) produced a conditioned secretion of 60 drops, (*b*) produced 30 drops, and the two applied simultaneously produced 90 drops. As an example of summation, this is not only perfectly simple, it is simply perfect. In contrast with this kind of simple summation are the results

[5] Of course it is possible that adaptation to the two signals separately would not result in adaptation to the combination of the two. This would make an interesting experiment. It would also be interesting to compare the adaptation to single and double signals.

of the present study. If we statistically compare the response to the double signal with the *sum* of the two single responses in each case we find that according to method (1) above, the mean of the sums is 4.00 (twice the average previously found, of course), the sigma is 11.49, the correlation is naturally the same as before, and the critical ratio of the difference over the sigma of the difference is 1.42. By method (2) the mean is 3.50, the sigma 10.01, and the critical ratio 2.32. By (3) the mean is −5.00, the sigma 54.67, and the critical ratio 2.50. These figures involving the sums of the single responses are, as should be expected, not as significant as were the differences between the single responses themselves and the responses to the double signals. In view of the smallness of the group of subjects, however, it seems probable that with more data they would become even more significant. At any rate it is probably true that the double signals tend to produce a response even greater than the sums of the responses to the single signals. Therefore this phenomenon should probably be thought of as facilitation rather than as summation. Thus we have gone beyond Pavlov, although we had just corroborated with human subjects his statement, based on the study of dogs, that the response to a compound stimulus, the components of which have already been conditioned, is greater than the conditioned response to either of the components.

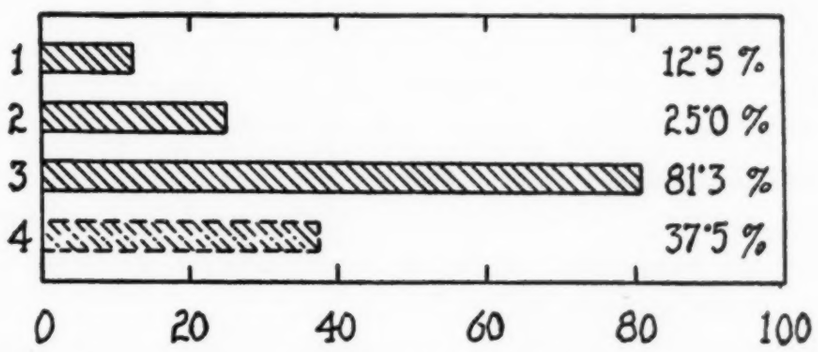

FIG. 4a. Comparative values of single and compound stimuli. Percentage of conditioned stimuli which evoked finger movement in case of (1) a buzzer, (2) a vibrator, (3) buzzer and vibrator together. Subjects had been conditioned to buzzer and vibrator separately but not together. (4) is the sum of (1) and (2), or the maximum theoretically expected in view of Pavlov's results.

The above results are substantiated by analysis of the frequencies of finger reactions. On the third day when 2 single and 2 double signals were given to each subject in balanced order, 7 of the 8 subjects responded to a total of 81.3 percent of the double signals, while but 3 subjects responded to only 18.8 percent of the component signals. On the fourth day, when to each subject 28 double and 4 single signals were offered, although not in balanced order, 62.5 percent of the double and only 21.9 percent of the single signals elicited responses. On the fifth day, with stimuli presented as on the fourth, 56.7 percent of the double and 9.4 percent of the single signals elicited responses. This is clear evidence of the summation or facilitation of response tendencies resulting in more frequent responses to the compound signal.

Differentiation

Pavlov says (**12**, 144) that it is easy to make a stimulatory compound into a negative or inhibitory stimulus, while its components applied singly maintain their positive effect. To quote, ". . . if a conditioned reflex to a compound stimulus was established as described above [conditioning of compound before presentation of components singly] it was easy to maintain it in full strength and at the same time to convert its individual components, which gave a positive effect when tested singly, into negative or inhibitory stimuli. This result was obtained by constant reinforcement of the compound stimulus, while its components on the frequent occasions when they are applied singly, remain without reinforcement. The experiment can be made with equal success in the reverse direction, making the stimulatory compound into a negative or inhibitory stimulus, while its components applied singly maintain their positive effect."

An attempt was made in the present study to produce just such a situation as the latter. We presented to the 8 subjects of our main group, who had been conditioned to the vibrator and to the buzzer on their first two days of experimentation, compound stimuli of buzzer and vibrator together, without reinforcement, as well as component stimuli with reinforce-

ment. On their third day these subjects received, in somewhat random order, 16 to 19 unreinforced compound stimuli and from 10 to 14 reinforcements of each component (also 2 unreinforced presentations of each component). On each of their fourth and fifth days they received 28 compound stimuli, 6 reinforcements of each component, and 2 unreinforced

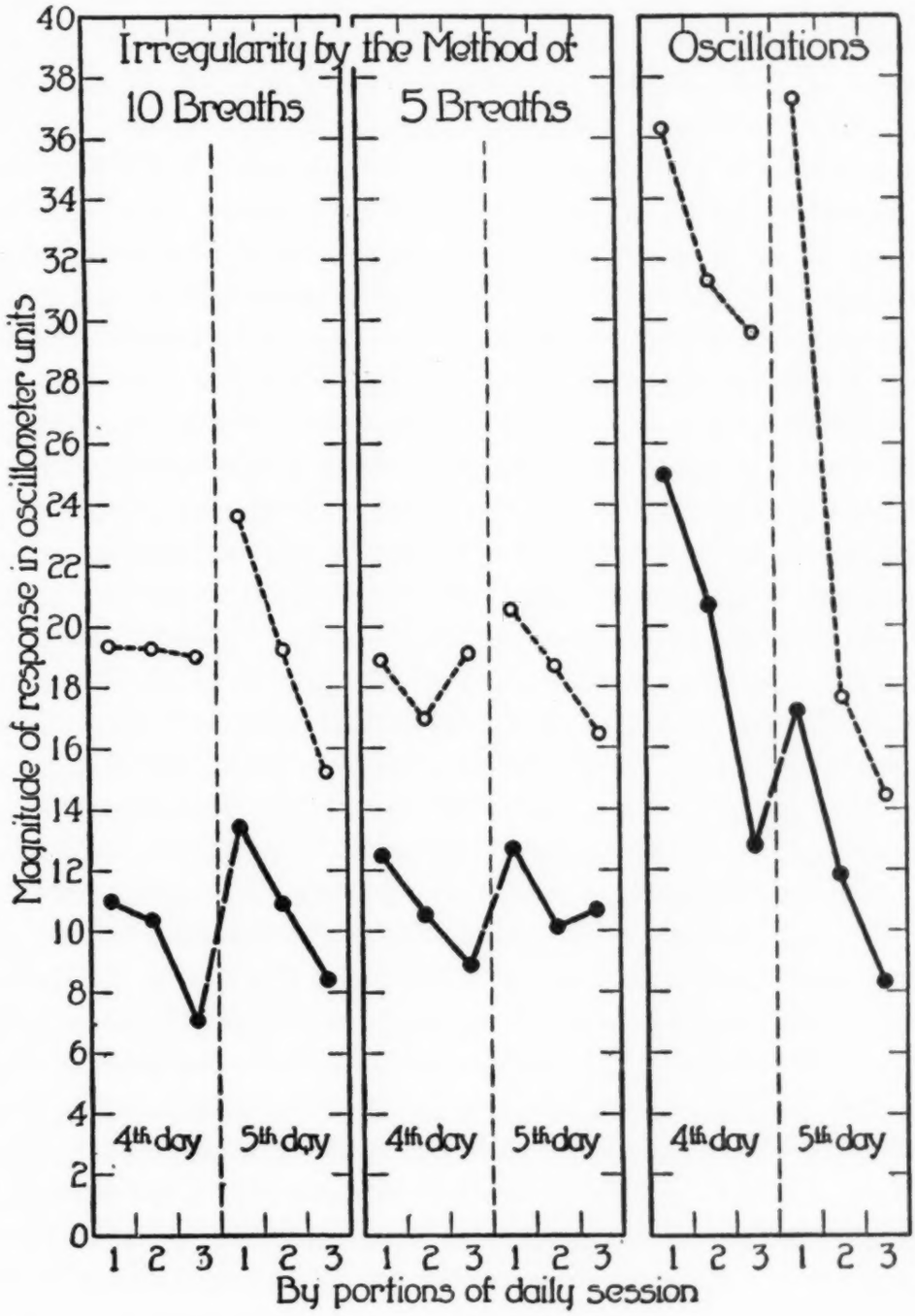

FIG. 5. Extinction of double signal responses.

presentations of each component. Twenty of these 28 compound stimuli were given in immediate succession, in order to hasten experimental extinction (the compound or double signal was never reinforced in this study). The results for the fourth and fifth days are represented in the curves of Fig. 5. The solid curves represent the values of the response to the double signal at the beginning of the session, near the middle, and near the end. The first point on each curve is derived from responses to the first 5 of the 20 double signals which were given in succession, the second point is from the last 5 of these 20, and the last point is from the last 6 double stimuli of the session. Each point on the dotted curve is an average of the responses to 4 reinforced single stimuli on each subject. The first 4 were given just before the series of 20 double signals, the next 4 just after, and the last 4 were interspersed with the last 6 double signals. This is plotted as a control on the possibility of a general fatigue effect or a process of extinction which might involve the responses to signal plus shock, as well as to signals alone. This process does not explain the extinction appearing in the responses to signals, since the two processes do not always follow the same course, as can be seen on the fourth day. In general, the solid curves show a decrease in the effect of the double signal during the experimental session. The dashed portion indicates spontaneous recovery from the end of the fourth day to the beginning of the fifth. These statements apply equally to the study of the oscillations of the respiratory tracing and to both methods of studying its irregularity.

The additional fact for which we are looking is a lowering of the values of the fifth day over those of the fourth. We find evidence of this not in the irregularities but only in the oscillations and even here the extinction is far from complete. It is possible, even probable, that a longer continuation of the experiment would have produced more complete extinction. The important consideration, however, is that the situation is not so easily produced as one would expect from Pavlov's experience; that even after from 72 to 75 repetitions of the unreinforced compound signal spread over 3 days, experi-

mental extinction had not been definitely accomplished. A possible explanation of the difference between our result and that of Pavlov should be sought in the experimental differences. (1) Pavlov used dogs, whereas we used college students. If conditioning is a learning phenomenon, the students should be superior to the dogs and the difference should be the opposite of what it is, since we usually find that students form conditioned responses rather quickly as compared with dogs. (2) From the statement of Pavlov quoted above, it is not clear whether he reversed the training order when he made the experiment 'in the reverse direction,' but we suppose that he did not. We did. We conditioned our subjects to the component (single) signals before presenting them with the compound (double). We should think that this would make the difference the opposite of what it is, since it ought to be easier to extinguish the compound if it had never been reinforced, as in our study, than if it had been definitely conditioned, as may have been the case in Pavlov's study. Hence, here again, we find no explanatory factor. (3) As in most of the Russian work, the stimuli in Pavlov's experiment were continuous or lingering stimuli, such as tones, lamps, metronomes, or rotating objects, which impinged upon the animal for at least several seconds before the reinforcement was given. Pavlov emphasizes the importance of this interval and says (**13**, 92) that attempts at backward conditioning prove difficult and unstable. The intervals in the present study were small fractions of a second and the stimuli, both signals and shocks, were correspondingly brief. This constitutes a fundamental difference between the two procedures. If our subjects had had several seconds after a signal before it became necessary to respond, the results might have been quite different.[6] Surely, if the signal were continuous during this long interval, the human subject would soon learn the difference between a single and a double signal, and the difference between a double signal and a signal plus a painful shock. He would even have time to formulate in verbal terms a differential attitude toward the two situations. He would no

[6] In a conversation, Mr. W. C. Shipley contributed to the development of this idea.

longer make such erroneous statements concerning the stimuli as are reported in this study, statements which indicate an inability in some instances to distinguish one stimulus from another, or to perceive two at the same time. His task would be a simple learning problem, but it would be just that far removed from what we commonly call the reflex level. One of our purposes in studying conditioned responses in psychology is to reduce learning to the level of conditioned reflexes. If we are to do this we must recognize the same differences (if any) between conditioned reflexes and other conditioned responses as between reflexes and responses in general. This is the reason we work with short intervals: to reduce the subject's opportunity to verbalize the situation and solve it on an extra-reflex basis. One need not even suspect the dog of verbalization, however, in order to suggest that he too would have greater difficulty with the problem if it were presented in terms of split-second stimuli and brief intervals. There is a serious question as to whether he would solve it at all. It would be instructive to see many of the canine conditioned reflex experiments repeated under these conditions.

The Effect of the Interval Between Tests

Just as the interval between double stimulations in the training series was varied to avoid temporal conditioning, so the interval between test stimuli was varied to avoid anticipation and temporal determination of the response. It was found that the magnitude of the conditioned response varied with the length of the interval which preceded the conditioned stimulus. Thus in the series of 20 double signal stimuli which were given in succession in an attempt to extinguish the double signal, when the responses are classified according to the length of the interval preceding each, the result is as indicated in curve *C* of Fig. 6.[7]

[7] This curve is based on the increased irregularity of 10 post-stimulus inspirations and each point is the difference between the corresponding points of curves *A* and *B*. The values on curve *C* for the 45, 60, 75, and 90 second intervals are respectively 1.9, 11.3, 11.5, and 15.6; for *A*, 19.8, 15.0, 13.0, and 14.3; and for *B*, 21.7, 26.3, 24.5, and 30.0. *N* is 96, 48, 80, and 96.

Since the measure of response is the difference between pre- and post-stimulus readings, the question arises as to whether the increase in response with increase in interval is due to actual increased activity of the subject or is merely a reflection of decreased pre-stimulus values. For this reason,

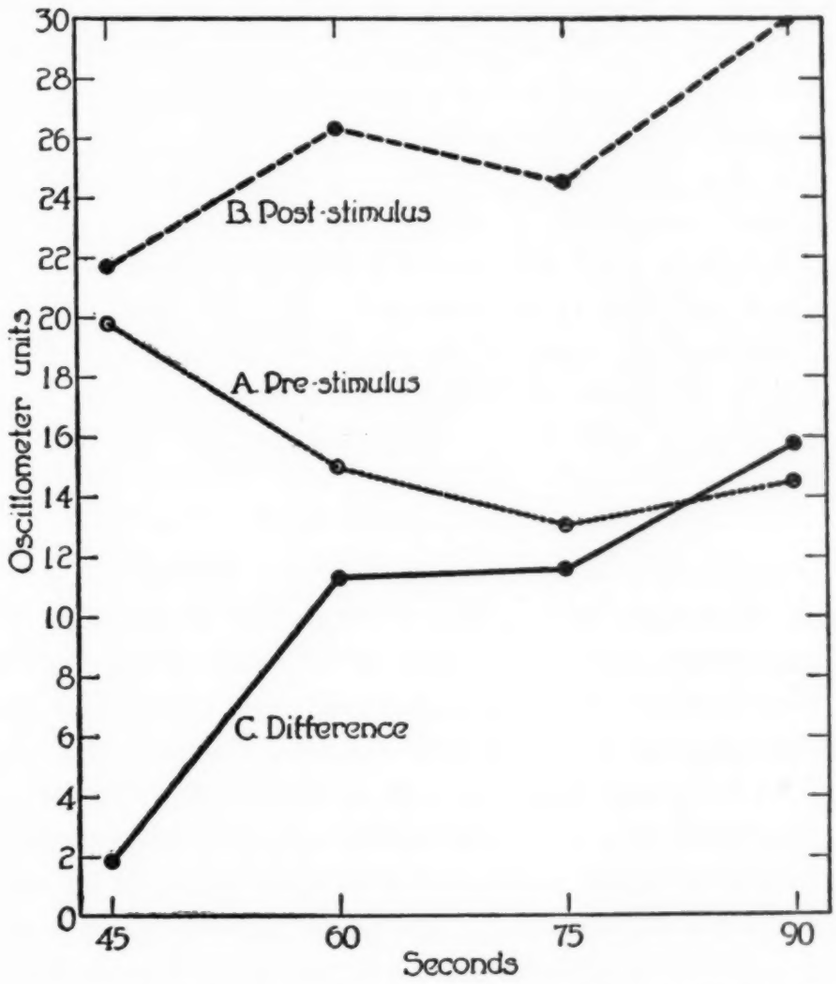

FIG. 6. The relation between amplitude of response and length of interval preceding the stimulus: (*C*) Increased irregularity of 10 inspirations.

the pre- and post-stimulus values are plotted in curves *A* and *B*. While the pre-stimulus values do decrease with increased interval, this is not a more determining factor than the absolute increase in the post-stimulus values. While the breathing becomes more regular during a long interval than during a short one, it also becomes more irregular in response to a signal following a long interval than to a signal following a short interval. This relation between the response and the

interval resembles the result reported by Wendt (**18**, 38). He obtained 4.5, 4.6, and 5.0 mm of conditioned quadriceps thickening after intervals of 30, 60, and 90 seconds. He attributes this increase to 'complex factors of expectation at the longer intervals.' The present result is also probably due to an increase in expectation as the interval lengthens, and throws favorable light on the concept of conditioning as a function of an attitude of expectancy on the part of the subject, as suggested by Freeman (**5**, 534 ff.).

That the effect of the interval applies especially to the conditioned responses is shown by analysis of data on the control subjects who were not conditioned but who were given the signals without reinforcement. In this case, where there is no conditioning, there is no effect of the interval, the curve comparable to curve *B* being practically flat, with values of 20.9, 18.4, 22.4, and 20.9. This strengthens the expectation hypothesis.

Verbal Reports

The subjects' verbalizations were for the most part spontaneous, although each subject was asked at the end of each of the last three sessions to give an account of the experiment in his own words. If the account was very sketchy or meager, he was prompted by questions such as, "What else did you notice?" In some cases in which the subject reported the vibrator, let us say, and omitted to mention other stimuli, he was asked if he heard a noise, or a bell, or if he felt any shocks. The questions were intended to be as non-suggestive as possible of answers which might be expected. The information which was given in answer to questions was recorded separately from that which was volunteered.

The most interesting features.of the subjects' reports are the discrepancies between them and the objective facts of the experimental situation. These 'erroneous' reports were obtained from all 8 subjects of the main group, and can be fitted into two classes: first, those which tell of combinations of stimuli which were not presented and second, those which deny stimuli which were presented.

In spite of the fact that no shock was ever given with the

double signal, 6 of the 8 subjects reported that it was. Two of these men (subjects 4, 26) said this occurred only once or twice; two (subjects 9, 23) said it happened several times; one (subject 19) said the double signal was accompanied by the shock as often as not; and one man (subject 10) even said it was usually accompanied by a shock, and that the shock seldom came in any other combination. This man reported the shock in this combination each day for three days.

In the second class fall such items as the report that for one whole session (of the last three experimental days) no shock accompanied the buzzer (subject 21). Subject 26 similarly reported that for one whole session no shock accompanied the vibrator. These two together with three others (subjects 1, 9, 23), reported reception of the shock without any signal at some time during the three sessions. Three men (subjects 1, 4, 21) definitely said that there were no double signals during a whole session, while one (subject 26) was not sure. Subject 1 reported the absence of the vibrator for a whole session. An examination of the kymograph records shows not only that the instruments were operating in all cases, but also that in many cases the subject responded to the stimuli which he testified against, and the response was so clear-cut as to be observable from mere inspection of the record. The nature of the differences between subjects' reports and the objective records indicates that when the subjects reported shock where no shock was given, they had learned to attach the shock to or identify it with the signal. This we might include under the term conditioning—the subject makes a (verbal) response to the signal as if it were a shock. The reports of shock alone when no shock was given alone may also have been produced by a signal alone which the subject perceived as a shock, and to which he responded as such. The following spontaneous observations of subjects seem to throw light on these assumptions. Subject 23 said, "Immediately after the buzzer I feel just about the same as if I had been shocked." Subject 26 reported, "Sometimes I thought I got a shock, and then I

realized that it was only the buzzer." [8] Scott's subjects made similar comments, supporting a response interpretation of feeling (**15**, 127 ff.).

On the other hand, erroneous reports of shock alone may have occurred in case of a shock accompanied by a signal, the subject being unable to report both. It is as if the shock sometimes prevents the perception of the signal. This would also seem to be the situation when the shock-signal combination is denied: Subject 21 reported shock alone, buzzer alone, and the absence of buzzer-shock combination on the same day. It is also possible but not probable that the signal determined the subjects' response to the exclusion of the shock. The exclusion by one signal of the verbal response to another signal occurring at the same time would, however, explain the denials of the double signal. In order to make a detailed study of such selective responses to simultaneous stimuli, one should provide for continuous automatic recording of the subject's report concerning each stimulus situation as it occurs. In spite of the limitations of our technique, however, it seems significant for the above consideration that subject 4 volunteered the observation, "I sometimes felt the shock alone, and *then* I realized that there had been the vibrator along with it." [9]

[8] In the first class of reports above, we may say that the subject thought he got a shock and then *failed* to realize that it was only a signal. These observations are interesting in comparison with the reports of Wendt's (**18**, 62) subjects who made conditioned responses without realizing that the natural stimulus had been omitted and those who made such responses and then voiced astonishment at having responded without the natural stimulus. This leads us to an example which has not been reported. In the fall of 1930 Mr. D. G. Marquis, Mr. R. R. Sears, Mr. R. S. Sackett, and the writer made a preliminary attempt to condition the abdominal reflex to a buzzer. The buzzer was sounded at irregular intervals of from 30 to 90 seconds. This was followed by a stroke on the subject's abdomen with a stirring rod at an interval of the writer's reaction time. The subject became drowsy, and when the natural stimulus was omitted and the conditioned reflex (which was different from the unconditioned reflex) occurred, he roused suddenly and exclaimed, "What did you do to me that time?" He did not notice that the signal was present, but thought the unconditioned stimulus was different owing, no doubt. to the fact that the response was different.

[9] Other observations which may be of psychological importance are: Subject 1, "The vibrator made the shock feel worse—the shock seemed stronger when accompanied by the vibrator." Subject 26, "Sometimes I got nervous waiting for the shock. I was expecting it and sort of tightened up."

Summary

1. Increases in amount and irregularity of breathing, as well as movement of the finger, were conditioned to a buzzer and a vibrator, based on the reactions to faradic stimulation.

2. The buzzer and vibrator produced small respiratory changes without shock at the beginning of the experiment.

3. With control subjects, these original responses rapidly disappeared upon repetition of the unreinforced signals.

4. This process of negative adaptation continues to a negative response value, indicating that adaptation may be, at least temporarily, an inhibition phenomenon. Experiments are suggested.

5. The question is raised as to whether the value of the original (pre-reinforcement) response to the signal should be subtracted from the post-reinforcement value to determine the magnitude of the conditioned response value. It is suggested that the post-reinforcement response should be taken at its face value, provided the number of presentations of the signal is such as would have produced complete adaptation without reinforcement.

6. Differences in variability and definiteness between respiratory changes following simple stimuli and those following strong emotional stimuli are pointed out.

7. An unsuccessful attempt was made to discover whether subjects would respond to one signal simply because of having been conditioned to another.

8. A perfect example of certain conditioning phenomena is shown with a warning against accepting such good examples as typical.

9. 'Learning curves' of the acquisition of conditioned response are shown, one on 8 experimental subjects and one on 32 unselected subjects.

10. Pavlov's results on summation were corroborated in that a compound signal, the components of which had been conditioned, produced greater conditioned responses than either of the components alone.

11. Our results differed from those of Pavlov in that our compound signal produced responses even greater than the *sum* of the conditioned responses to the components alone.

12. The question is raised as to whether adaptation to two signals separately would result in adaptation to their combination.

13. The differentiation of a compound signal from components which have been conditioned was found much more difficult than should be expected from Pavlov's teachings and was not definitely accomplished within the limits of the experiment.

14. A possible explanation for the difference between this result and that of Pavlov is found in the durational aspects and temporal relations of the stimuli.

15. It is suggested that human subjects would solve on an extra-reflex basis the differentiation problem as set by Pavlov, and that dogs would fail to solve on any basis the problem set our subjects.

16. The relation between the magnitude of the response and the length of interval preceding the test stimulus supports the expectation hypothesis of conditioned response. The lack of such relation in the case of subjects who have not been conditioned confirms this support.

17. Spontaneous verbal reports of subjects support the expectation hypothesis and a response interpretation of feeling.

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AN ALL-OR-NONE CHARACTERISTIC IN THE ELIMINATION OF ERRORS DURING THE LEARNING OF A STYLUS MAZE

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Problem

The problem of this experiment was to measure the time spent in the individual culs-de-sac of a stylus maze and to analyze the temporal course of elimination. The usual time measurements in studying the acquisition of an act of skill are made in terms of the total act, and the learning curves for time are plotted, therefore, only for total time. Important as such measurements are, they give no insight into the distribution of the time spent over the parts of the pattern which is being acquired. An adequate description of the learning requires that we know in detail the time relations within the pattern as well as the total time taken in elimination and fixation. The present report is concerned only with the learning curves for time in the case of individual culs-de-sac.

Apparatus and Method

The maze pattern used was that employed by Warden (Pattern I) in his study of primacy and recency.[1] In this pattern only a single type of alley is included and the true pathways are homogenous with the alleys. In the maze used in this experiment, each alley extends 1¾″ at right angles to the true path and each has a right-angled arm extending 1″ in the forward direction. There are 5 alleys on each side of the true path, distributed in the order: R, L, L, L, R, R, L, R, R, L. The uniform construction of the alleys and the equally uniform character of the sections of the true path require a relatively homogeneous set of movements.

The maze pattern was cut from a rectangle of bakelite 10¼″ × 11½″. The paths, which were cut entirely through, were ¼″ wide and ¼″ deep. The maze was mounted upon a base consisting of a brass plate covered by a rectangle of bakelite ⅜″ thick. In this base, brass contact points were placed at a distance equal to the diameter of the stylus (2 mm) within the entrance to each section of the true path.

[1] C. J. Warden, Primacy and recency as factors in cul-de-sac elimination in a stylus maze, *J. Exper. Psychol.*, 1924, 7, 98–116.

These contact points were filed flush with the floor of the maze and were very seldom, if ever, detected by a subject. They were, however, placed at the entrance to each section of the true path, as well as in the alleys, in order to control their possible use as cues. The contacts were circular, equal in diameter to the width of the path, and were inevitably touched by the blunt point of the stylus in crossing them. The stylus consisted of a brass rod 2 mm in diameter, with a composition handle and a small binding post at the upper end. The contact points and the stylus were connected with a recording pen and a battery, the circuit being completed when the tip of the stylus came in contact with a point. The wire from stylus to pen was soft, pliable, light of weight, and ran through a small swivel pulley suspended from the frame of the maze screen. The subject, who could not, of course, see the attachments on the other side of the screen, was unable to perceive any pull of the wire on the stylus as the latter was moved.

The recording device consisted of two electrically operated fountain pens, which marked on a long endless band of paper moved by a weight motor. One pen was in circuit with the stylus and the contact points, the other with a Jacquet chronograph set for contacts in fifths of a second. Only the contact points in the alleys were connected with the lead to the fountain pen. When the subject entered an alley, the recording pen was actuated, again if he entered the right-angled arm of the alley, and again as he came out of each. The end of the band on which the record was made was behind the screen, to the front and side of the maze. As each alley was entered, the experimenter recorded above the record the number of the alley. It was, then, possible to count, from the time line immediately below, the time, in fifths of a second, spent in each section of each alley.

The maze was learned to a criterion of three successive perfect trials by 30 college students, none of whom had had any previous maze experience. The customary instructions were read to the subject and care was taken that they were thoroughly understood. Records were made of total time, trials and errors, as well as of the time spent in each alley.

Treatment of Data

The data obtained by the methods described make it possible to draw a learning curve for each subject and each alley, plotting time spent in the alley against successive trials. This gives, for each of the three methods of plotting to be described presently, 30 separate curves for each of the 10 alleys. An adequate generalization can scarcely be made from such a diversity, and the curves for each alley cannot, on account of the widely differing numbers of trials required for elimination, be added as they stand to give a composite curve. We have, therefore, employed the Vincent curve [2] technique. The learning curves for time were first drawn for each subject and each alley with trials plotted on the abscissa. These curves were then transformed into Vincent curves by dividing

[2] S. B. Vincent, The function of the vibrissæ in the behavior of the white rat, *Behav. Monog.*, 1912, 1, pp. 81.

each curve into twentieths.[3] The Vincent curve data were then pooled to give a composite curve which shows the time spent in any given alley at each successive twentieth of the trials required for learning. This method has the great advantage of reducing to a common base a large amount of highly variable data and of making possible, thereby, a general quantitative interpretation. It has the disadvantage at the same time, that it masks many individual characteristics of the data and provides a final result some distance removed from the raw data from which it sets out. If, however, one is careful in his interpretations never to lose sight of the characteristics of the original data and always to see the individual curves behind the generalized one, there can be little question of the validity of the results of the method. It distorts the data no more than, if as much as, any measure of central tendency.

There are several ways in which, by the use of the Vincent curve technique, the problem of the measurement of the temporal aspects of cul-de-sac elimination may be approached.

Method I. The first and most obvious procedure is to draw a curve showing the time spent by each subject in each alley, plotting successive trials on the abscissa and time on the ordinate. This is the method usually employed when the total time per trial is being studied. In the case of single alleys, however, the procedure is less simple. The major complications are summarized in the following three divisions. (*A*) There occur many trials in which a given alley is not entered and in which, therefore, its time-score is zero. The alley cannot be said to have been eliminated, since it is entered again on subsequent trials. The curve representing the time spent in successive trials will, thus, drop to the

[3] The data for all subjects and for all alleys were plotted on a proportional scale such that every curve was drawn on an abscissa 200 mm in length. This was greatly aided by tables showing the exact divisions of the abscissa for each number of trials from 10 to 100. We are indebted to Dr. Arthur W. Melton of Yale University, who devised these tables and who was good enough to provide us with copies and to examine the ways in which we have used the Vincent curve technique. When all curves are plotted on abscissas of equal lengths, it is an easy matter to divide the curves into 1/20s or into any other desired fraction. It may be noted that this method renders transformation errors constant in magnitude.

abscissa several times during the course of learning. When this curve is divided into twentieths for the purpose of obtaining data for a Vincent curve, the record at certain points will be either zero or a quantity considerably below any amount of time actually spent at any entrance. A composite of individual curves drawn in this way represents in a smoothed fashion the composite result of both entrances and failures to enter up to the final criterial elimination of the alley. It fails completely to show the course of the time spent at the actual entrances. It should be noted that this method must, of necessity, when there are failures to enter, indicate less time per trial than is actually spent, and since, in most cases, alleys are omitted somewhat oftener as learning progresses, it will tend to give the curve a greater irregularity, a somewhat greater slope, and smaller values than are actually present in terms of time per trial. That is, the curve is distorted by gaps during which trials are made but no time is spent in a given alley. Its sole advantage is that it represents the whole course of learning up to the criterial elimination of all errors. (*B*) Any given cul-de-sac may, for example, not be entered during a relatively large number of trials, only to be re-entered later in the learning. The question of what criterion to apply in such cases at once presents itself. We have assumed, for the purposes of our curves, that an alley is not eliminated until it shows no more entrances up to the point of three perfect runs of the entire pattern. It is quite conceivable that some other criterion is worth applying, such, for example, as three or five successive failures to enter. In defense of this it could be urged that we use such a criterion for the total pattern and can well use it for the parts. We have not employed it, however, because it has seemed better to study the elimination of each cul-de-sac in the total context of the maze, and therefore to consider it eliminated only when it was entered no more during the time required for the criterial organization of the total pattern. (*C*) Some culs-de-sac will, especially in the early trials, be entered more than once in a single trial, if retracings are permitted. We have permitted retracings because we wished to study the acquisition of the maze habit

under conditions which left the subject free to employ whatever movements the situation elicited, backward as well as forward. We have, when plotting by this method, taken, in cases of more than a single entrance per trial, the average time spent in the alley as a representative figure for that trial.

Method II.—A second mode of analysis is to consider only those trials upon which the alley is actually entered, and, as well, to treat each entrance, where there is more than one per alley per trial, as itself a trial. This gives a curve which represents the time spent in successive entrances and which disregards, as irrelevant, each individual trial required for the fixation of the total pattern. It has the advantage of presenting a clear picture of the actual course of the time spent in actual entrances and is, thus, a complement of the first method. Its disadvantage is that it treats each alley as a separate unit entirely divorced from the total pattern of which it is a part and from what has happened between entrances.

Method III.—A third procedure is to combine the time spent in all 10 alleys into a single curve showing the average amount of time spent per entrance per trial. This is analogous to the usual curves for time, save that time spent in the true path is excluded.[4]

Each of these methods has certain advantages and certain disadvantages and data obtained by the use of each will be presented. It should be obvious that the form of any learning curve is a function of the character of the variables plotted against each other, and that there is no absolute or *real* curve, in the sense of one which represents learning as such or in general. We can, in the present problem, however, plot the data in the three ways described and attempt to abstract certain general features from them.

Results

The Vincent curves for total time spent in successive trials in the learning of a maze usually fall rapidly at first, then

[4] In all of these computations the alleys have been treated as units and no differentiation has been made between time spent in the main arm of the alley and time spent in the right-angled arm which projects in the forward direction. In all of the curves shown the mean times of the first trial has been plotted on the zero ordinate. As a result each curve has 21 plotted points.

more slowly, and approach the abscissa asymptotically. The initial rapid decrease is taken to represent the subjects' period of adjustment to the new maze situation and of orientation toward the goal, while the much slower decrease in the latter half of the curve represents the gradual elimination of errors and integration of right responses. Reasoning from the curve for total time one would expect a similar curve for individual alleys. Such a curve would mean that progressively less and less time was being spent in the alley, until finally it was no longer entered.

When, however, the curves for time spent in the culs-de-sac at successive trials were drawn according to Method I, the majority failed to follow the curve for total time. A composite of the 30 curves for each alley has been drawn, together with a composite of the 10 curves for the individual alleys. Eight of the 10 curves show a sharp initial drop, covering roughly the first 3/20ths of the trials, then a gradual decline for about 9/20ths, followed by a somewhat irregular fluctuation, but with no tendency toward a decrease, for the remainder of the learning period. Elimination, thus, occurs suddenly. We have reproduced in Fig. 1 the curve (Alley 2) in which the initial decline is sharpest and in which the decrease in time beyond the third twentieth is most negligible. During the last 17/20ths of the learning, the subjects entered Alley 2 for virtually an equal amount of time, on the average, when they entered it at all. The curves for Alleys 1, 2, 5, 6 and 7 have this general form, and those for Alleys 3, 8 and 10 approach it. The curves for Alleys 4 and 9 have a gradual slope throughout the greater part of the learning period. We have shown, also, in Fig. 1, the curve for Alley 9, which deviates farthest from the tendency represented by Alley 2, and in Fig. 2 the composite of the 10 curves for the individual alleys. This is a composite, therefore, of 300 individual curves.

The curves of the general form of that shown for Alley 2 have two clearly marked phases. The first is the sharp decline which covers roughly the time during which the subject is exploring the alley during the initial trials and before, pre-

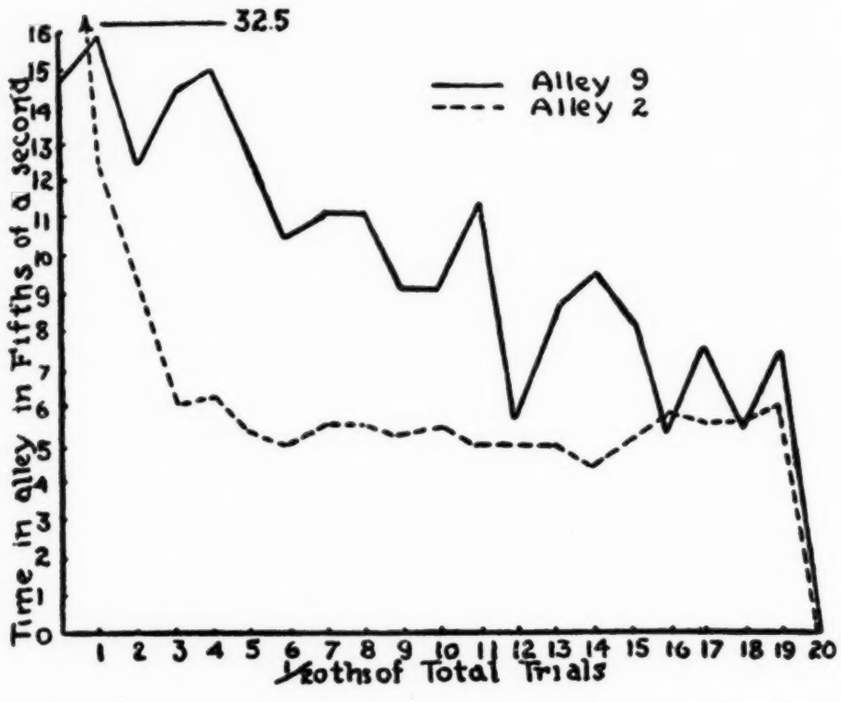

FIG. 1. Vincent Curves—By Method I, which includes all trials.

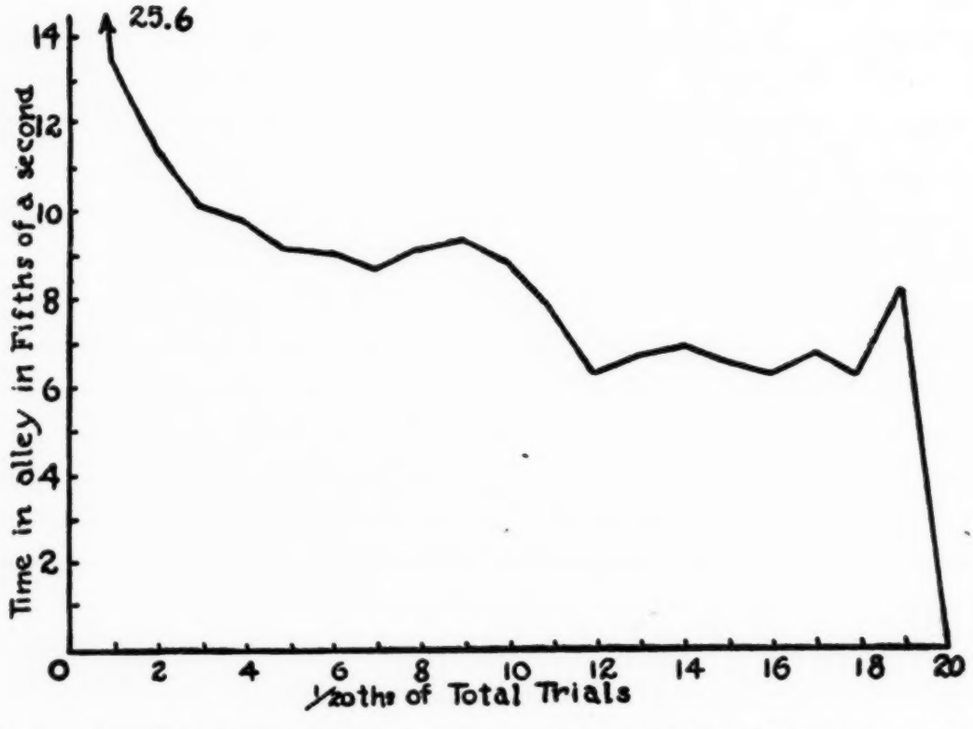

FIG. 2. Composite Vincent Curves for 10 alleys—By Method I which includes all trials.

sumably, a clear orientation toward the goal has been developed. The second, is the rough parallelism with the abscissa during the remainder of the learning period. The first phase of the curve is what previous studies of learning have led one to expect. The second phase is unexpected. It is possible for a subject to remain in an alley for lengths of time varying between any physically possible time and a single fifth of a second. Our measurements do not permit accurate determination of smaller lengths of time than the latter. It would, therefore, be possible for the learning curves for time to fall from any given initial height to a point one-fifth of a second above the abscissa at the 19/20th of the learning period. The fact that there are often, at varying points during learning, single entrances which are as short as one- or two-fifths of a second proves the possibility of making briefer entrances than the plotted averages represent and of measuring them, if made. Previous measurements of the progress of learning would lead one to anticipate an asymptotic approach very close to the abscissa. This does not appear. Instead, the curves remain at a height which represents more than enough time to enter the alley to its full length and return.[5] This is true up to the last point at which an error is made.

These curves represent, after the initial decline, a kind of all-or-none elimination of errors in terms of time. That is, on the average, a given alley will be entered for a period of time which varies relatively little, and which is significantly greater than the time required for entrance only as far as the contact point at the mouth of the alley, or it will not be entered at all. Conversely, it is eliminated with temporal completeness or not at all.

It has already been said that five of the ten alleys do not as clearly yield this type of curve. Alleys 3, 8 and 10 approach it; Alleys 4 and 9 are of the type shown by the curve for the latter. In these curves the initial decline extends much farther over the total learning period, and although there is

[5] Whether the subjects explored any alley completely, or where the time was spent beyond the contact points, our present records do not show. In majority of cases, however, both arms were explored to and through the last entrances. There is no particular point in the elimination process where only first arm tends to be explored.

no decrease over the last two-fifths of the period, the time fluctuates more than in the case of alleys of the first type discussed. In spite of these differences, however, the same general conclusion is indicated in so far as there is no gradual approach to the abscissa. Only the relative constancy of the time spent is absent. This lack of constancy is not, however, a significant exception when it is remembered that the units are fifths of a second and that the fluctuations are, therefore, very small in absolute amount.

The composite of the curves for the 10 alleys (Fig. 2) shows a somewhat steep initial decline over the first 2/20ths of the learning period, a more gradual decline to the twelfth twentieth, and only minor fluctuations from that point to a slight rise just preceding elimination. The total decrease in time from the first to the twelfth twentieth is from 13.4 to 6.2 fifths of a second, or slightly more than one second. This very small amount, in conjunction with the absence of a decrease beyond the twelfth twentieth, supports the conclusion that, in a maze of this pattern, elimination is, temporally, an all-or-none phenomenon in the sense that an alley will, during the latter part of the learning, be entered for a roughly constant time, which is much longer than the time required for the briefest possible entrance-exit movement, or it will not be entered at all. Conversely, it will be completely eliminated, temporally, or not at all.

Certain additional facts support this conclusion. A majority of individual curves for the raw data, before they are transmuted into Vincent curves, show clearly this all-or-none characteristic, and such transmutation by Method I masks to a considerable extent the features of the original curves. In the description of Method I it was said that this method tends to give to the curves a greater irregularity and smaller values than are present in the untransmuted curves, on account of its inclusion of trials upon which no entrances were made. There are more entrances in the first than in the second half of the learning period, and this fact will account for the lower values of the second half of the curve and for at least a part of the antecedent slope. The difference is not great, but it is

present, and the empirical findings for which it accounts are, likewise, of small magnitude. This method, moreover, consistently yields time values which are lower than the time actually spent when the alley is entered and hence yields a time curve which is nearer than it should be to the abscissa, thus masking somewhat the all-or-none effect.

We may return briefly to the statement that a majority of the individual curves show the characteristics which we are discussing. We have gone over each of the 300 individual curves and have rated each one with respect to whether it possessed the all-or-none character described. The number showing it is, for each alley in order, as follows: 25, 20, 17, 21, 22, 21, 18, 16, 16 and 17. The number showing at least some tendency toward an asymptotic approach is, for each alley in order: 4, 6, 9, 9, 3, 7, 9, 4, 13 and 6. The remainder were questionable and were not rated in either class. No curve fails to show the sharp drop at the end. We have asked other psychologists to rate the curves for individual alleys and they tend to ascribe clear all-or-none character to more curves than do we.

The already mentioned disadvantages of Method I are corrected by Method II, according to which each entrance is plotted separately and only entrances are plotted. Neither the regularity of the results nor the absolute time values can, by this method, be affected by failures to enter an alley. Characteristic curves are yielded by Alleys 1 and 10. The curves for Alleys 4 and 9 deviate farthest from the all-or-none tendency. We have, therefore, reproduced (Figs. 3 and 4) the curves for these four alleys. The same general features which were revealed by Method I appear again. In the curves for Alleys 1 and 10 there is a relatively sharp initial decline, followed by a more gradual one extending roughly to the end of the first half of the learning period. During the second half of the period the curves parallel the abscissa. The curves for Alleys 4 and 9 show the same initial decline, but remain, thereafter, nearly parallel with the abscissa until well into the second half of the learning period, when a small drop appears. At the final elimination, however, the two

curves are 3.9″ and 2.6″ above the x-axis, and there is nothing in either curve to contradict the conclusion that there is no asymptotic approach, and that, when elimination occurs, it occurs suddenly.

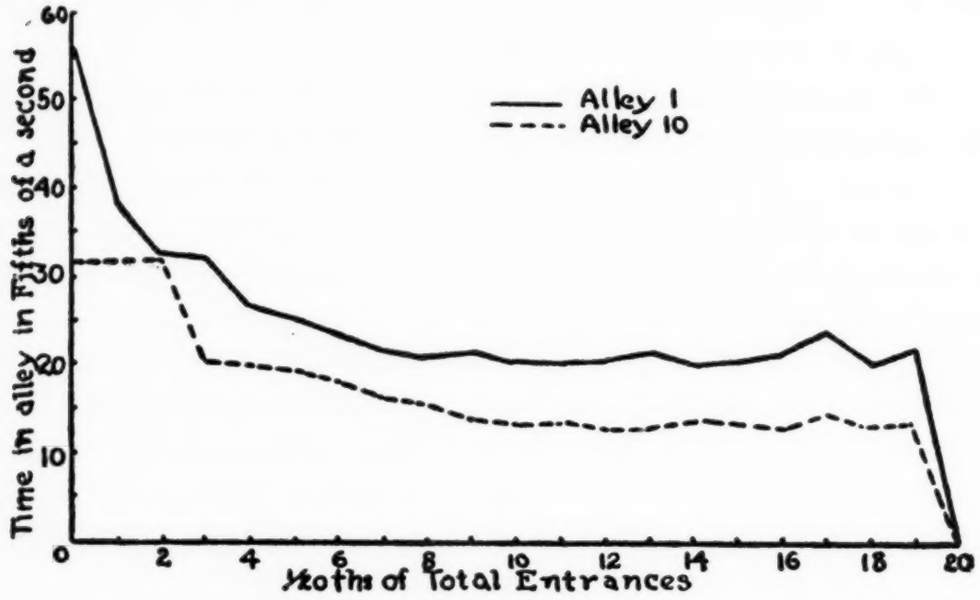

FIG. 3. Vincent Curves—By Method II in which each entrance is plotted separately and entrances only are included.

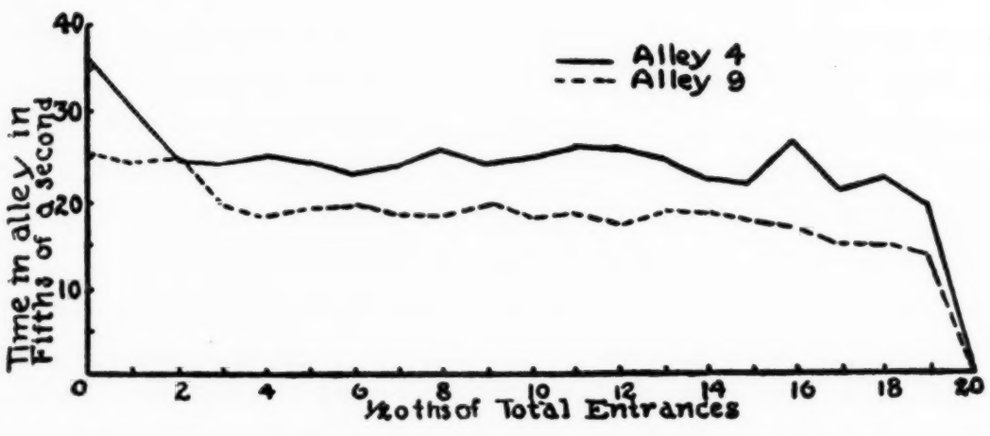

FIG. 4. Vincent Curves—By Method II in which each entrance is plotted separately and entrances only are included.

The amounts of time spent in the alley are faithfully represented by this method and are much larger than those yielded by Method I. After the initial decline, covering the first three or four twentieths, the decrease in time spent is very small, amounting in most cases to less than a second.

It seems necessary to conclude that, more clearly by this method than by Method I, elimination proceeds temporally according to an all-or-none principle.

It is necessary to take every precaution in the use of the Vincent curve technique to insure that one's results are not artifacts of averaging. As an additional check, we have tabulated separately the data for each group of subjects making the same number of entrances into any alley. These curves have shown uniform agreement with the composite curves already presented. In order to conserve space we shall not present them, but shall give, instead, the mean times in fifths of a second for the entrances during the last half of the learning period for a few representative cases, particularly for small numbers of entrances, since in these cases interpolation error is greater than when the number of entrances is large. Four subjects entered Alley 1 fourteen times each and their times during the last 7 entrances were: 9.7, 22.2, 14.7, 15.0, 9.7, 20.5, 12.5. Two subjects entered Alley 2 twenty-two times and their records for the last 11 entrances are: 6.7, 21.5, 15.0, 17.7, 14.5, 6.7, 12.5, 13.2, 9.7, 12.7 and 15.5. Four subjects entered Alley 8 eight times and their records are: 9.5, 18.6, 17.8 and 15.8. Four entered Alley 10 ten times with records as follows: 15.5, 17.1, 18.7, 17.1 and 15.8. Clearly the tendency of these combined raw values is the same as that of the averages of the Vincent curve values.

It should be stated that the division of our curves into twentieths has not, by any method, greatly exceeded the average number of entrances. Had it done so, our results, while not vitiated thereby to any degree, would contain a greater interpolation error, since a division greater than the average number of entrances would increase the interpolation error for the rapid learners and decrease it for the slow ones.

The question of the statistical properties of the means which our curves represent at once arises. We have computed the sigmas of the means for several alleys and we shall present these sigmas for Alleys 1 and 10. The consistency in the form of the curves for different alleys and by different methods is, however, the best guarantee of their significance

and little is added by a presentation of sigmas. We shall, therefore, list the sigmas in serial order from 1 to 20 and leave the means to be read from the graphs. The sigmas of the means of each twentieth in the case of Alley 1 are: 7.56; 4.90; 4.65; 4.46; 1.91; 2.00; 2.11; 1.98; 2.17; 2.43; 2.84; 2.75; 2.54; 2.49; 2.47; 2.17; 2.49; 2.50; 2.76; 2.88 and 3.34. Similarly, the sigmas of the means of each twentieth are, for Alley 10: 2.97; 6.42; 4.15; 2.73; 2.08; 2.17; 2.11; 2.08; 1.90; 1.40; 1.36; 1.25; 1.29; 1.30; 1.42; 1.52; 1.44; 1.84; 1.01; 1.34 and 1.54. There is nothing in these sigmas to indicate that the form of the curve is a statistical artifact.

The third manner of treating the data is to obtain for each subject the average time per entrance for the 10 alleys combined, to plot this for each subject, convert the curves into Vincent curve values, sum the latter and draw from the result a composite curve. By this method the Vincent curve technique is applied only to the average time per entrance into all of the alleys by each subject. Each point on the curve represents 300 original records, *i.e.*, records for 30 subjects and 10 alleys.

The curve which results from this method is shown in Fig. 5. It is characterized by a very small and gradual slope through almost its whole course, but the total amount of the decline from highest to lowest point is small. The striking fact is that it nowhere approaches the abscissa and is 2.5 sec.

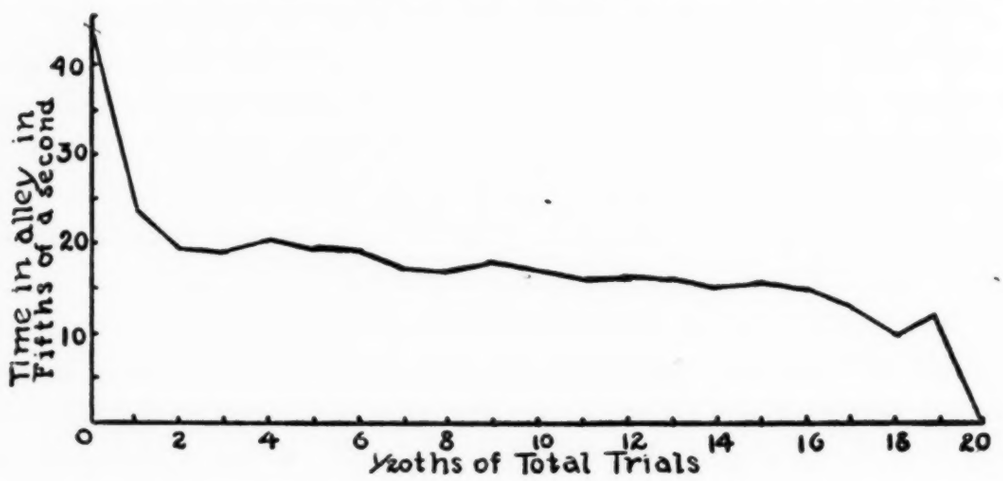

FIG. 5. Vincent Curve for the Average time spent per entrance per trial in all 10 alleys (Method III).

above it at the last twentieth before elimination. The individual curves clearly have a tendency to drop very little and many of them either parallel the abscissa or rise slightly as learning progresses. This method shows elimination to be sudden, but the time spent preceding elimination decreases, though very slightly, throughout the learning period.

It should be noted that, by all the methods used, the extent of the decline during the early part of the learning period is exaggerated. When the learning period is divided into as many as 20 parts, this exaggeration is produced in large degree by subjects who require relatively few trials for learning. The period of initial adjustment, when thus divided, appears to have been longer than it actually was.

It cannot be argued that the parallelism of the latter part of the curves with the abscissa is a function only of the time physically necessary for an entrance-exit movement at normal maze-running speed. The time necessary for such a movement should be a constant for all alleys, since all are of the same dimensions, yet each alley has its own temporal constant which differs from that of each other alley. Moreover, the time spent in each alley is more than would be required for the briefest possible entrance-exit movement by the ordinary subject.[6]

Upon a first examination of the form of these curves it is possible to consider them as being the same in significance as the curve for total time (Fig. 6). When, however, the elimination of the individual alley is compared with the learning of the total maze, it is seen that, even though the curves for the two were of identical form, the interpretation would still be different. The time curve for the total maze includes, until the criterial elimination of errors, the time spent both in the making of errors and in the forward traversing of the true path. Time decreases as more and more errors are elimin-

[6] Although our records do not show the movements of the subject between contact points, there is some evidence that the alleys are treated as wholes in most cases. The percentages of complete entrances, *i.e.*, entrances into both segments of the alley, are for the 10 alleys as follows: 92.7, 86.1, 86.3, 92.0, 80.2, 87.0, 89.6, 85.4, 83.8 and 82.9. There are, thus, only small percentages of incomplete entrances. The partial entrances show no preference for any one portion of the learning, but are distributed throughout its course.

ated and, which often happens, as speed increases. Finally, when all culs-de-sac have been eliminated, a certain time is physically necessary for the traversal of the true path. The curve for the total maze may approach, but it can never reach,

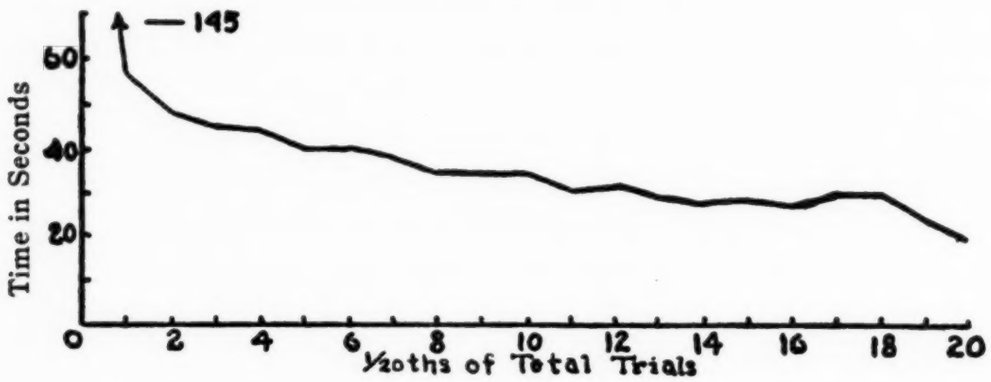

FIG. 6. Vincent Curve for total time (blind alleys and true path).

the abscissa. It is possible, on the other hand, for the time curve of the individual alley to fall very slowly until it approaches within 1/5″ of the abscissa before the final elimination when it must reach the abscissa. The curve for total time can never reach it, the curve for individual alley-time must reach it. The discovery that the latter strikes the abscissa suddenly appears all the more important since the approach might have been a slow and gradual one.

To demonstrate a perfect all-or-none elimination, the time curve should be straight and parallel with the abscissa up to the last twentieth of the learning period. Against the background of the manysidedness of both maze pattern and individual differences, such a curve could hardly be expected, no matter how completely elimination may be all-or-none in character. Each entrance into a cul-de-sac occurs set in the contex of the subject's reactions to the rest of the maze, of his shifting sets, of his changing organic conditions, and of many other factors. That all subjects could spend a uniform time in each cul-de-sac is scarcely possible, since other factors beside that of all-or-none character must bear upon every act. These bring it to pass that at one time a subject moves faster than at others, and in varied fashions alter the time spent in the cul-de-sac. It is not the purpose of this paper to analyze

these factors, but it is necessary to point out that they are present to mask or to prevent the clear appearance of an all-or-none elimination. Since, even under these conditions, the average time spent in the individual alley during the second half of the learning period is so nearly uniform, and since elimination is relatively sudden, the conclusion that elimination occurs in an all-or-none manner seems to be necessary. The complexity of the phenomena involved, however, is so great that a conclusion of such seemingly great importance must be accepted only tentatively and with suspicion prior to further investigation. While, therefore, an effort will be made to examine the implications of such an all-or-none elimination and to defend it, its tentative character must always be remembered.

It is necessary to ask what such an all-or-none phenomenon implies in this context, and here we shall attempt to present certain possibilities and it must be emphasized that they are possibilities only. In the physiology of muscle and nerve, the all-or-none law refers to the responses of individual nerve or muscle fibres, and it may seem an impassable distance from such simple responses to the complex behavior of alley elimination. The culs-de-sac of a stylus maze are single units only from the arbitrary standpoint of maze construction and of the counting of errors, and the simplest entrance-exit movement on the part of the subject involves a relatively complex organization of nerve-muscle patterns and of associative processes. This may not, however, mean that the two all-or-none phenomena must necessarily be qualitatively different. A single muscle fiber is a unit only with respect to the more complex organization of the total muscle. With reference to its own biochemical constituents it is not a unit. The associative response of passing, instead of entering, an alley, while it involves a relatively complex integration, is a unit within the context of the pattern of behavior in the maze and may be given a place, logically, within the range of the same concept. By this no speculation regarding the physiological basis of the observed fact is intended. What is meant is that complex integrations of the associative order may show

the same characteristics and be subsumed under the same concept without logical difficulty, at least, until the conditions of the latter phenomenon are more adequately investigated. On the other hand, it may be urged that the all-or-none law as physiology knows it refers to the factor of intensity, while the phenomenon we have discovered has to do with time only. To this it may be replied that the time factor gains its importance through the stimuli which it brings to bear on the individual and through the development of the responses which it makes possible, and that, therefore, it is of the same order as the intensity law of physiology. The framing of a satisfactory hypothesis must wait upon further experimental fact, and we have been concerned here only with the brief discussion of an important possibility.

It is worth while to inquire briefly what implications the all-or-none character of elimination may have for the learning curve and for theories of fixation and elimination. The characteristic curve of learning in which time is plotted against trials, shows an asymptotic approach to the abscissa. If the elimination of errors follows a temporal all-or-none law, how are the gradualness of the total curve and the suddenness of the individual curves to be reconciled? The answer is the same in kind as that given in the case of the graded response of a muscle. The gradualness is a function of the innervation of more (or of fewer) individual fibres in the case of the muscle, and of the elimination of more and more errors in the case of the learning curve of the total maze. One may ask, however, why, if such is the case, the curve does not show a stepwise form. The most immediate reply is that the usual learning curves for total patterns are combinations of many curves for individual alleys. The points of final elimination do not coincide and the stepwise character is ironed out in the composite. There are other factors also which mask such a stepwise appearance. Among them are the different times spent in different alleys, the variable of true path time and of speed of movement, together with the fact than an alley need not be entered at each run until it is eliminated and hence that at successive runs different alleys with different individual times may be entered.

A detailed analysis of the significance of the all-or-none phenomenon for theories of fixation and elimination should wait upon verification with other facts. Only a brief examination need be given it now. It seems to offer evidence against any theory which assumes a gradual elimination of individual errors and a general and progressively increasing overlapping of responses. Theories which employ frequency as in itself an eliminating factor are at least not supported. The fact of suddenness of elimination makes it appear that some other factor is at work, a factor which frequency, perhaps, gives an opportunity to act. Certainly it places a temporal limitation upon frequency, a limitation which defines its temporal extent for each individual error. Whatever factors are at work to produce the final and complete elimination, must in order to bring it about, operate over a certain length of time in any given alley. This time is not in the second half of the learning period, susceptible to gradual reduction. A given frequency of entrance thus means, during this period, a given number of entrances for a constant length of time. Whether the phenomenon is related to refractory phase in associative processes one can do no more than speculate.

If we accept it tentatively as fact that each alley has a constant entrance time during the later portion of the learning period for that alley, it is a plausible hypothesis that this is because whatever cues or sensory consequences or effects are present require a certain minimum time in which to operate. It represents, that is, a temporal limen for effect. Why this should vary with alleys in different serial positions is a question. Some more general context factor involving the total pattern, redintegrative in character, is perhaps in action.

These tentative inferences from specific facts may be nullified by further research, but whatever the fate of particular inferences, it seems likely that an examination of the elimination of individual errors and the fixation of individual correct responses offers a significant mode of attack upon the organization of total learned patterns.

SUMMARY

The time spent at each entrance into the alleys during the learning of a 10-alley Warden maze by 30 college students, has been measured. The temporal course of elimination has been studied by three different methods of treating the results. The resulting curves have the following characteristics:

1. There is, by Methods I and II, a relatively sharp initial decrease in time during the first two or three twentieths of the learning period. This is followed by a very slight decline from the second or third to the tenth or twelfth twentieth of the learning trials.

2. During roughly the last half of the learning period there is no further decline in the time spent in the alley. The time spent, thus, remains a constant at a value which is very greatly above the time required for an entrance-exit movement. In every case the final elimination of the alley occurs suddenly.

3. The curve yielded by Method III falls very slowly throughout most of its course, but is, at the end, high above the abscissa, and elimination again occurs suddenly.

These facts are interpreted to mean that error elimination, under the conditions employed, proceeds according to an all-or-none principle which may be stated thus: After the brief period of initial adjustment and orientation and particularly during the last half of the trials taken to learn, an alley is entered for a period of time which is on the average nearly a constant for that alley, or it is not entered at all. This period of time is much longer than would be required for a simple entrance-exit movement. The law may also be stated in terms of elimination to read: In the learning of a stylus maze, an alley is eliminated completely, in terms of time spent, if it is eliminated at all. The temporal magnitude of the error does not, during the last half of the learning period, decline gradually; it is a constant until it disappears suddenly and entirely.

The learning curve of the individual alleys is thus found to have two aspects: (*A*) the initial orientation and solution-finding aspect, and (*B*) the final elimination aspect. The

time curve for the former drops rapidly at first and then more slowly until, at about the middle of the learning period, this phase has passed completely into the elimination phase, the all-or-none characteristic which we have been describing.

Whether other forms of learning, or even whether the learning of other maze patterns, follows this all-or-none principle cannot be said in the absence of direct measurement. In so far as the principle applies, however, it is of importance for theories of elimination and fixation and for the understanding of the whole of learning. Certain of its implications have been discussed.

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SOME FACTORS INFLUENCING VOLUNTARY AND REFLEX EYELID RESPONSES

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The relationship between reflex and voluntary activities is an old and difficult problem. The advantages of the eye-wink for the investigation of this relationship have previously been pointed out (see Telford and Anderson, 1932). In a previous study considerable similarity between the reflex and the voluntarily reinforced and inhibited eye-wink responses was reported. With all three types of responses there was evidence of a refractory period following activity. With continued stimulation, a progressive diminution of the amplitude of response took place under all three conditions. There was some evidence that practice on the voluntary level also influenced the purely reflex activities (Telford and Anderson, 1932).

Allison (1932) found that the reaction-time of voluntary winking was considerably influenced by the length of the time-intervals between responses while the reflex winks had the same latencies when the interval between winks was varied from $\frac{1}{2}$ to 4 seconds. For the voluntary winks the shortest reaction-times were obtained with a 2-seconds interval between responses. Shortening the time interval to 1 and $\frac{1}{2}$ or lengthening it to 4 seconds resulted in slower reactions. These results are suggestive of Telford's (1931) findings for voluntary finger reactions and judgments of linear magnitudes with varying time-intervals between responses.

Allison's failure to find any difference in the latencies of the reflex responses with varying intervals between stimuli is surprising in view of the evidence for the existence of a refractory period which may last for as long as 6 seconds and the further finding that responses occurring in the relative refractory period have a latency longer than normal (see Gotch,

1910; Lucas and Adrian, 1917; Forbes, Ray, and Griffith, 1923; Gasser and Erlanger, 1925; Dodge, 1927; Telford and Anderson, 1932). The above mentioned results were obtained when the various time-intervals between responses occurred in an irregular order. It was thought that it would be well to supplement these findings with results obtained under comparable experimental conditions, but using a regular recurring stimulus and varying the length of the time-interval in successive series instead of following an irregular order as heretofore. In a previous study (Telford and Anderson, 1932) some evidence of periodic variations in the amplitude of eye-lid responses was obtained and it was thought that such variations might show up more clearly if regular intervals were used.

Reflex Eyelid Responses

In view of these possibilities we have repeated some of our former work using regular in place of irregular time-intervals and the present paper is a report of the results obtained.

The apparatus was the same as that used in our earlier work, except that only one instead of two hammers was used and slight improvements in the recording apparatus were made.[1] The procedures in the two studies are essentially the same except for the order of time-intervals.

Each subject was comfortably seated before the apparatus, his head placed in the head-rest, the recording device fastened to his eyelid, and then instructed to fixate an object three feet away and directly in his line of vision. He was instructed to sit perfectly passive. The stimulus consisting of a rubber-tipped wooden hammer striking a plate glass 3 cm in front of the eyes was controlled by the experimenter. All of the subjects except two used in the present study were sophomore students taking general psychology at the University of North Dakota. The other two subjects (C and D of Tables XI and XII) were the junior and senior authors respectively.

The time-intervals between stimuli were $\frac{1}{2}$, 1, 2, 4, and 6 seconds. In an attempt to equalize practice effects for the long and short intervals the order 1, 4, $\frac{1}{2}$, 6, 2 was used. Following this procedure four subjects were used and the record of a series of responses consisting of one round of the kymograph was obtained from each person at each time-interval. The amplitude of each response was measured and a summary

[1] A complete description of this apparatus will be found in our earlier study; see Telford and Anderson (1932).

of the results are given in Tables I and II. Our electrical recording device was such that the records obtained have about 1½ times the amplitude of the actual response. All of the tables are based on the uncorrected readings.

TABLE I

MEAN AMPLITUDE OF REFLEX EYE-WINK RESPONSES WITH DIFFERENT TIME-INTERVALS BETWEEN STIMULI (Order of Intervals was 1, 4, ½, 6, and 2)

Interval in Secs.	Subject A			Subject B			Subject C			Subject D			Average		
	M *	σ	N	M	σ	N	M	σ	N	M	σ	N	M	σ	N
½	5.25	1.88	141	1.36	3.97	187	2.55	3.43	235	1.63	4.48	113	2.95	3.44	169
1	5.99	1.93	176	6.16	2.41	137	2.76	6.40	222	1.47	1.94	116	4.10	3.17	163
2	5.21	4.36	172	0.56	1.29	232	5.77	2.59	127	1.95	1.99	116	3.37	2.56	162
4	7.52	1.99	114	6.83	1.66	129	4.92	2.13	98	4.03	2.64	102	5.83	2.11	111
6	7.63	1.56	101	3.84	1.81	151	1.30	1.47	84	3.86	5.42	99	4.16	2.57	109

* In this and all succeeding tables dealing with amplitude of responses, M is the mean amplitude of the responses in cm, σ is standard deviation of the distribution and N is the number of responses.

TABLE II

DIFFERENCES AND RELIABILITY OF DIFFERENCES BETWEEN SUCCESSIVE MEANS OF TABLE I

Intervals Compared	Subject A			Subject B			Subject C			Subject D			Average		
	D *	σ of D	D / σ of D	D	σ of D	D / σ of D	D	σ of D	D / σ of D	D	σ of D	D / σ of D	D	σ of D	D / σ of D
½ & 1	.74	.21	3.52	4.80	.36	13.33	.21	.49	.43	−.16	.46	.35	1.15	.36	3.19
1 & 2	−.78	.36	2.17	−5.60	.22	25.45	3.01	.49	6.14	.48	.26	1.85	−.73	.32	2.28
2 & 4	2.31	.38	6.05	6.27	.17	33.00	−.85	.31	2.74	2.08	.32	6.50	2.46	.28	8.78
4 & 6	.11	.24	.46	−2.99	.20	14.95	−3.62	.27	13.41	−.17	.59	.29	1.67	.32	5.22

* D is difference between the means. Minus quantities indicate that the second of the two intervals being compared is smaller than the first.

These results indicate that on the average the smallest responses are obtained with a ½-second interval between responses. As this interval is increased to 1 second, the amplitude of the response increases, a slight decrease is found at the 2-seconds, which is in turn followed by an increase at the 4- and another drop at the 6-seconds intervals. Table II shows that all of these differences, with the possible exception of the

decline at the 2-seconds interval, are statistically reliable. An examination of the results for the different individuals of Table I shows that the results for the different subjects vary considerably. Subject A deviates from the group results in showing a continued increase instead of a decrease when the interval between responses is increased from 4 to 6 seconds. This increase is probably not significant, as Table II indicates. Subject C shows an increase in average amplitude of response as the interval between responses increases to 1 and 2 seconds, with declines at the 4- and 6-seconds intervals. The responses of subject D show an insignificant difference between the $\frac{1}{2}$- and 1-second intervals with increased amplitudes at the 2- and 4-seconds and a slight but statistically unreliable decrease at the 6-seconds interval. The results from subject B are similar to those of the average.

It will be seen that the group results are quite similar to those previously found and suggest the existence of a refractory period which is followed by a period of magnified responses and a subsequent decline.

The records indicated that negative adaptation takes place very rapidly under continuous stimulation. This suggested that some of the irregularities and differences obtained were due to the particular order of intervals used in the experimental series. For example, the small average amplitude of responses at the 2- and 6-seconds intervals is probably due to their coming late in the series; similarly the large amplitude at the 1-second interval is possibly due to its early position. Consequently the two individuals (B and D) who showed the greatest variability were selected as subjects for a second sitting. This time the order of intervals used was 1, 4, $\frac{1}{2}$, 6, 2, 6, 2, $\frac{1}{2}$, 4, and 1. In this way practice effects are more nearly equalized. The results from the second sitting of these two subjects are given in Tables III and IV.

It will be noted that these results are much more regular and in keeping with the group results of Tables I and II. Subject B shows a constant increase in amplitude of response as the interval between responses increases and subject D does likewise except for a slight decline at the 6-seconds interval.

TABLE III

MEAN AMPLITUDE OF REFLEX EYELID RESPONSES WITH VARYING TIME INTERVALS BETWEEN RESPONSES

Interval in Secs.	Subject D			Subject B		
	M	σ	N	M	σ	N
½	2.90	1.97	150	3.26	2.96	194
1	3.16	2.81	248	3.95	3.14	168
2	4.10	2.03	202	6.49	2.10	174
4	5.25	2.09	139	10.08	2.85	116
6	4.68	1.68	107	10.39	2.01	104

TABLE IV

RELIABILITY OF DIFFERENCES BETWEEN SUCCESSIVE MEANS OF TABLE IV

Intervals Compared	Subject D			Subject B		
	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$
½ and 1.....	0.26	.24	1.08	0.69	.32	2.16
1 and 2.....	0.94	.24	3.92	2.54	.30	8.47
2 and 4.....	1.15	.24	4.79	3.59	.22	16.32
4 and 6.....	−0.57	.24	2.46	0.31	.26	1.19

Of course, these subjects had considerable practice in the first sitting which may have influenced the results, consequently it was thought desirable to take additional subjects through the series following the same schedule of time-intervals as a further check on these results. The results from 5 additional subjects are given in Tables V and VI.

It will be noted that the only difference between the results of this table and those preceding is the continued increase in amplitude of responses as the time-interval between stimuli is increased from 4 to 6 seconds. These results indicate that part of the decline at the 6-seconds as well as the excessive height of the 4-seconds intervals in Tables I and II are due to their respective positions in the experimental series. The individual results are similar to the averages except for the slight declines in amplitudes at the 2-seconds interval for subjects A and E and at the 6-seconds interval for D. The writer still feels that some of the irregularities of the individual results are due to the rapid decline in the amplitudes of the

TABLE V

MEAN AMPLITUDE OF REFLEX EYE-WINK RESPONSES WITH DIFFERENT TIME-INTERVALS BETWEEN RESPONSES

Interval in Secs.	Subject A			Subject B			Subject C			Subject D			Subject E			Average		
	M	σ	N	M	σ	N	M	σ	N	M	σ	N	M	σ	N	M	σ	N
½	2.85	5.12	155	3.47	4.78	156	1.02	2.26	163	1.36	1.86	172	2.78	2.09	172	2.30	3.22	164
1	5.23	6.38	172	5.13	4.77	211	1.61	3.07	210	1.68	2.03	173	4.34	1.63	155	3.61	3.58	184
2	5.24	3.96	165	6.35	3.25	136	3.10	3.13	156	2.16	1.77	134	2.80	1.74	155	3.93	2.77	149
4	8.37	5.37	123	7.49	2.56	101	5.28	2.80	121	4.89	1.90	105	4.48	1.76	117	6.10	2.88	113
6	9.64	2.46	103	8.43	2.02	92	7.17	1.24	100	4.73	1.67	98	5.12	1.86	102	7.02	1.85	99

TABLE VI

DIFFERENCES AND RELIABILITIES OF DIFFERENCES BETWEEN MEANS OF TABLE V

Intervals Compared	Subject A			Subject B			Subject C			Subject D			Subject E			Average		
	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D
½ & 1	2.43	.64	3.80	1.66	.50	3.32	.59	.28	2.11	.32	.21	1.52	1.56	.21	7.43	1.31	.36	3.64
1 & 2	−.04	.58	.07	1.22	.43	2.84	1.49	.33	4.55	.48	.22	2.18	−1.54	.19	8.11	.32	.35	.91
2 & 4	3.13	.58	5.40	1.14	.38	3.00	2.18	.36	6.06	2.73	.24	1.14	1.68	.21	8.00	2.17	.35	6.20
4 & 6	1.27	.80	1.59	.94	.33	2.85	1.89	.28	6.75	−.16	.25	.64	.64	.24	2.67	.92	.32	2.88

responses in the first few series. Even our revised procedure is not entirely satisfactory for equalizing practice and fatigue effects. The irregular order of intervals used in the previous study was much better in this respect. However, as far as the grouped results are concerned, it seems that it makes little difference whether a regular or irregular procedure is followed in administering the stimuli when reflex responses are studied. Under both conditions a refractory period is found, the length of which varies with different individuals. What little evidence we find of periodic fluctuations in amplitude of the reflex responses can probably be accounted for on the basis of differential practice effects at the various time intervals due to the particular order of time-intervals followed in the experimental procedure.

The Eye-wink as a Voluntarily Reinforced Response

Three persons were taken through the series of reactions, in the same manner as described in the preceding section, except that they were instructed to wink voluntarily each time the stimulus was given. The same order of time-intervals as those used in obtaining the results of Tables III, IV, V, and VI were used. The results are given in Tables VII and VIII.

These results are exceedingly irregular. The amplitudes of these responses, particularly those of subjects B and C, suggest periodic fluctuations. These curves show alternately high and low points throughout their course. Subject A shows a steady decline in amplitude of response as the interval between responses is increased from $\frac{1}{2}$ to 1, and 2 seconds. This is followed by a slight increase at 4 and a subsequent drop at 6 seconds. The graph of the combined results of all three (which means little because the differences tend to cancel each other) is almost a straight line except for slightly increased amplitudes at the $\frac{1}{2}$ and 6-seconds intervals.

In view of the more rapid decline in the amplitudes of these voluntarily reinforced responses under continuous stimulation, it was thought desirable to attempt still further to equalize practice effects for the different time-intervals.

TABLE VII

MEAN AMPLITUDE OF VOLUNTARILY REINFORCED EYE-WINK RESPONSES WITH VARYING TIME-INTERVALS BETWEEN RESPONSES

Interval in Secs.	Subject A			Subject B			Subject C			Average		
	M	σ	N	M	σ	N	M	σ	N	M	σ	N
½	10.33	1.23	140	18.51	1.15	135	13.06	1.15	128	13.97	1.18	134
1	9.02	2.11	108	17.54	1.61	115	10.36	1.95	146	12.31	1.89	123
2	8.64	1.45	100	18.69	1.23	127	11.70	1.95	180	13.01	1.54	136
4	8.93	1.68	103	18.34	1.17	108	9.00	2.85	140	12.09	1.90	117
6	8.47	1.10	99	19.73	1.94	94	11.51	1.68	98	13.24	1.57	97

TABLE VIII

DIFFERENCES AND RELIABILITY OF DIFFERENCES BETWEEN THE SUCCESSIVE MEANS OF TABLE VII

Intervals Compared	Subject A			Subject B			Subject C			Average		
	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$
½ & 1	−1.31	.23	5.70	−.97	.34	2.85	−2.70	.19	14.21	−1.66	.20	8.30
1 & 2	−.38	.28	1.36	1.15	.18	6.39	1.34	.22	6.09	.70	.20	3.50
2 & 4	.29	.22	1.32	−.35	.16	2.19	−2.70	.27	10.00	−.92	.24	3.83
4 & 6	−.46	.20	2.30	1.39	.17	8.17	2.51	.29	8.66	1.15	.25	4.60

It seemed probable that the fluctuations in the preceding tables were due to differential practice effects. Consequently, the different time-intervals were so alternated that each interval occupied each position in the series sometime during the sitting. The kymograph was divided into 5 equal parts and enough responses at each interval were taken to complete a fifth of a round. Five rounds of the kymograph were used for each subject. This meant that one fifth of a round of responses were taken at each time-interval in the following orders: (1, 4, ½, 6, 2) (4, ½, 6, 2, 1) (½, 6, 2, 1, 4) (6, 2, 1, 4, ½) (2, 1, 4, ½, 6). Five subjects were taken through the entire series in this order. Their results are given in Tables IX and X.

These results are in striking contrast to those of the preceding section on reflex responses as well as previous data on voluntary processes. When an irregular order of intervals is used the curve of results for voluntarily reinforced responses

TABLE IX

MEAN AMPLITUDE OF VOLUNTARILY REINFORCED RESPONSES WITH DIFFERENT TIME-INTERVALS BETWEEN RESPONSES

Intervals in Secs.	Subject A			Subject B			Subject C			Subject D			Subject E			Average		
	M	σ	N	M	σ	N	M	σ	N	M	σ	N	M	σ	N	M	σ	N
½	4.80	2.17	143	5.72	2.63	143	9.18	2.76	211	6.26	1.12	159	8.01	.97	164	6.79	1.93	164
1	5.09	1.85	148	5.30	1.74	132	6.95	1.79	209	5.80	1.13	179	7.53	1.00	189	6.13	1.50	171
2	5.62	1.99	112	5.14	2.10	97	7.47	2.58	189	5.93	1.13	130	7.78	.83	133	6.39	1.73	132
4	5.29	1.97	143	5.94	1.74	115	7.81	2.15	159	6.01	1.10	153	7.81	.79	144	6.57	1.55	143
6	6.58	2.06	100	6.76	1.85	94	8.51	2.65	120	6.63	1.13	123	7.99	.95	125	7.29	1.73	112

TABLE X

DIFFERENCES AND RELIABILITY OF DIFFERENCES BETWEEN MEANS OF TABLE IX

Intervals Compared	Subject A			Subject B			Subject C			Subject D			Subject E			Average		
	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D	Diff.	σ of D	Diff./σ of D
½ & 1	.29	.24	1.21	−.42	.27	−1.56	−2.23	.14	15.93	−.46	.12	3.83	−.48	.10	4.80	−.66	.17	3.88
1 & 2	.53	.24	2.21	−.16	.26	.62	.52	.22	2.32	.13	.13	1.00	.25	.10	2.50	.26	.17	1.53
2 & 4	−.33	.25	1.32	.80	.26	3.08	.34	.28	1.21	.08	.13	.62	.03	.10	.30	.18	.20	.90
4 & 6	1.29	.26	4.96	.82	.25	3.28	.70	.32	2.19	.62	.14	4.43	.18	.11	1.64	.72	.20	3.60

follows closely that of reflex responses. The amplitude of responses increases as the interval between responses is increased from ½ to 4 and possibly 6 seconds. In the present data practically the opposite is found. Instead of the responses at the ½-second interval being the smallest, they are the second largest.

This result indicates that voluntary response to a regular recurring stimulus is, in some ways, quite different from simple reflex responses to the same stimulus. These differences however are not found when the responses are to stimuli occurring in an irregular order.

It seems quite evident that voluntary responses to a regular recurring stimulus, soon reduces itself to a matter of "beating time." This is evidenced by the fact that, if the stimulus is unexpectedly omitted at any regular interval, a wink occurs nevertheless. This is not true of the regular reflex responses and cannot happen in voluntary or reflex responses to a stimulus applied at irregular intervals. This means that the rhythmic responses come to be made, partially at least, to intra-organic conditions or stimuli and are determined by the 'set' or expectancy which the regularity of the previous responses has produced. This is probably what Miss Peak (1931, p. 39) has in mind when she refers to 'responses which have been initiated by the set rather than by the noise stimulus.' It is probable that these responses are quite different in their control from those of the other series. That is, they may consist of early voluntary responses which are, in most cases, initiated before the stimulus could produce the wink reflexly, the succeeding reflex response may be superimposed on the primary voluntary act. It is just as probable that our voluntarily reinforced responses to irregular stimuli, reported in the previous study, are in reality voluntary responses superimposed upon the primary reflex activity. Our recording device is not sensitive enough to make analyses and comparisons of the shape of the curves of the responses possible as Peak (1931) has done. Our results show only the total quantitative amplitudes of the responses and do not indicate any qualitative differences in the nature of the responses which may be present.

We have found no evidence for periodic fluctuations in excitability of the systems studied. Such evidence as there is of periodic changes in amplitude of response can be adequately accounted for on the basis of differential practice effects due to the order of intervals used in the experimental procedure, as has previously been pointed out.

The Eye-wink Response as Intraorganically Controlled

It has been shown that reading retards the normal rate of winking (Peterson and Allison, 1931) (Telford and Anderson, 1932). It was thought that this retardation might be due to the mental activity involved. In order to test this suggestion we have attempted to produce a state of mental activity without the visual accommodation, fixation, and eye-movements necessary to reading, being present. For this purpose subjects were given the task of multiplying mentally two-place numbers by two-place numbers as rapidly as possible. While doing this their eye-wink responses were recorded. The results obtained under these conditions were compared with those obtained (1) while the subject was reading and (2) while carrying on an ordinary conversation. The latter situation was considered normal and taken as a standard.

The eyelid responses of four subjects were recorded mechanically by means of especially devised apparatus. This apparatus consisted of a pair of 'spectacles' supporting a small lever attached to the subject's left eyelid and so arranged that closure of the eyelid brings the distant end of the lever into contact with a switch thereby closing the circuit and causing an electric marker to record on a kymograph drum the eyelid closure. Complete descriptions of this apparatus have been given by Peterson and Allison (1931), Cason (1922), and Allison (1930).

After the subject was comfortably seated and the 'spectacles' put on and adjusted, he was first given a book to read. After a few minutes which were allowed for the subject to become accustomed to the apparatus, the taking of the record began. Second, the subject was read two two-place numbers

which were to be multiplied mentally. When the correct answer was obtained another problem was given by the experimenter, etc. Third, the subject was engaged in ordinary conversation for a period of time. Each period lasted for about five minutes. The results are given in Tables XI and XII. These data are in terms of the length of the inter-blink periods in seconds.

TABLE XI

MEAN INTER-BLINK PERIODS OF FOUR SUBJECTS UNDER THREE DIFFERENT CONDITIONS

Occupation of Subject	Subject A			Subject B			Subject C			Subject D		
	M	σ	N	M	σ	N	M	σ	N	M	σ	N
Reading	32.12	8.45	13	13.87	3.70	27	4.14	1.67	172	5.48	2.57	129
M. Arith.	18.01	4.66	18	3.17	1.20	105	1.26	1.04	265	1.09	.93	172
Conversation	7.59	1.52	64	7.96	3.11	62	2.10	1.50	255	1.59	1.65	157

TABLE XII

DIFFERENCES AND RELIABILITY OF DIFFERENCES BETWEEN MEANS OF TABLE XI

Conditions Compared	Subject A			Subject B			Subject C			Subject D		
	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$
R. & M.A.	14.11	2.60	5.43	10.70	.73	14.66	2.88	.14	20.57	4.39	.24	18.29
R. & Con.	24.53	2.36	10.39	5.91	1.44	4.11	2.04	.15	12.69	3.89	.26	14.96
M. A. & Con.	10.42	1.07	9.74	4.79	1.25	3.83	.84	.12	7.00	.50	.15	3.33

These results show the longest inter-blink periods for reading and the shortest for the mental arithmetic. Only one individual (A) showed a longer inter-blink period for the mental arithmetic than for conversation. This individual performed the mathematical calculations with much greater ease than any of the other three subjects.

As a further check on these results, 36 general psychology students counted each other's eye-winks under the same three conditions as those described above. The counts were made in the general psychology laboratory of the University of North Dakota. The results from this group are given in Tables XIII and XIV.

TABLE XIII

MEAN NUMBER OF EYE-WINKS PER FIVE-MINUTE PERIOD

Occupation	M	σ	N
M. Arith.	54.43	3.99	36
Reading	14.36	2.71	36
Conversation	44.47	1.59	36

TABLE XIV

DIFFERENCES AND RELIABILITIES OF DIFFERENCES BETWEEN MEANS OF TABLE XIII

Conditions Compared	Diff.	σ of D	$\frac{\text{Diff.}}{\sigma \text{ of D}}$
R. & M. A.	40.07	5.85	6.85
R. & Con.	30.11	3.79	7.94
M. A. & Con.	9.96	3.25	3.06

It will be noted that these data corroborate the findings of the four cases whose results are given in Tables XI and XII. These data indicate that the decreased winking found during reading is not due to the mental activity involved but possibly to several other factors, such as the visual fixation involved in reading, the eye movements, and the fact that excessive winking interferes with the process of reading.

Miles (1931, p. 329–330) has recently pointed out several functions which winking serves in connection with perception in general. "It provides an opportunity for a new visual deal; adaptation is for the moment interrupted; stimuli are sharpened in outline and intensified in color. The more striking stimuli within the range of the visual field, with every wink, get a new opportunity to claim the attention of the observer. Negative after-images can be retained longer by occasional winking than by steady fixation. Winking which falls naturally between lines of reading fixations reduces the senseless visual blur. Finally, fluttering the curtains of the eye is one of the efficient ways of keeping awake and visually active." Besides these, Miles also points out that the upward motion of the eye-balls combined with the movement of the eyelids in winking serves several other purposes: "(1) The external recti secure relaxation from strain of convergence and fixation; the corollary of this is a renewal of the sensitive-

ness of the muscles to changes in tension and in their response to visual cues. (2) More complete darkening and thus resting of the retina is secured with the result of renewing somewhat its sensitiveness to stimuli, and of reducing the effects of visual adaptation. (3) Winking serves for a general massage of the eye, promoting venous circulation. (4) By double motion winking the eye avoids constantly pausing the tightly-fitting torsal edges of the lids at points on the apex of the cornea. This last consideration is quite an important thing to avoid. It is easy to notice that if an individual views a landscape or a picture through half-closed lids for twenty or thirty seconds, when he opens his eyes he will bat them several times in succession. The reason for this . . . is that the tightly fitting lid distorts the curve of the cornea. The rapid winking after such distortion is for the purpose of erasing or ironing it out."

These facts indicate that the moistening of the cornea in winking which has been supposed to be the principle result of the normal eye-wink response is only one of many. Similarily they suggest several possible sources of sensory control other than that of the drying of the cornea which Blount (1927) and Ponder and Kennedy (1927) have shown to be relatively unimportant in maintaining the normal, rhythmic eye-lid responses.

It would be interesting to record the eye-wink responses of individuals performing similar mental activities over longer periods of time in order to determine the effect of practice in the processes on rate of winking. It is possible that the increased rate of winking found while working mental arithmetic was due to the excitement and emotional tension involved. One fact which suggested this is that subject A of Table XI was the most complacent and did the problems the most rapidly of the four subjects in the group, and he also did not show an increase in winking rate while doing the mental multiplication. If the more rapid winking is due to the emotional element involved, it should decrease with increased practice in the process.

Summary

The amplitudes of reflex and voluntary eyelid responses to a *regular* recurring stimulus with varying time intervals between responses have been studied and the following results obtained:

1. The amplitude of reflex eyelid responses increases with increase in the length of the time interval between responses from $\frac{1}{2}$ to 4 and possibly to 6 seconds. This is in keeping with earlier works of the same kind in which an irregular instead of a regular time-interval between responses was used.

2. With voluntarily reinforced eyelid responses to the same stimulus the average amplitudes of the responses are greatest at the $\frac{1}{2}$- and 6-seconds intervals. The 1-, 2-, and 4-seconds intervals show smaller averages. These results differ considerably from those obtained under comparable conditions except that an irregular instead of a regular time interval was used. With the former procedure the greatest amplitude of response was obtained when 2 seconds intervened between successive responses. As the time interval was decreased to 1 and $\frac{1}{2}$ or increased to 4 and 6 seconds the responses decreased in amplitude.

3. These results show that as far as amplitude is concerned, reflex eyelid responses to a regularly and irregularly recurring stimulus and voluntarily reinforced responses are influenced alike by systematic variations in the time interval intervening between responses, while voluntary responses to a regular stimulus show almost opposite effects.

4. Reading retards, while working mental arithmetic increases the rate of the normal rhythmic eyelid responses of college students when the winking rate obtained while the subjects are carrying on an ordinary conversation is taken as normal.

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A CORRELATION BETWEEN ILLUMINATION INTENSITY AND NERVOUS MUSCULAR TENSION RESULTING FROM VISUAL EFFORT

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An analysis of the literature of visual sciences indicates a distinct gap in knowledge pertaining to the influence of intensity of illumination upon the functioning of the human being engaged in seeing. Researches in physiologic optics, involving intensity of illumination as an experimental variable, have been concerned largely with the determination of various visual thresholds. Less formal investigations in illuminating engineering have emphasized speed and precision of performing visual tasks as criteria of the beneficence of better lighting. In view of the importance of the conservation of human resources, it is evident that researches relating lighting and seeing have been confined to narrow channels. Visual functions have been extensively studied and attempts have been made to measure eye-fatigue. The authors have made various attempts to measure more deep-seated effects of visual work, but previously have not been successful in obtaining convincing quantitative data. In other words, we have taken into account the human-being aspect of the human seeing-machine. Most work in physiologic optics has not done so.

The purpose of the research discussed in this paper and its orientation in the general field of visual science are diagrammatically shown by reference to Fig. 1. This diagram was developed from a combination of quantitative measurements, qualitative observations, experience and philosophical considerations (**1**, **2**). It may be considered as a diagrammatic presentation of the operating characteristics of a human being performing a visual or visually controlled task. The vertical scale is in terms of human energy and, for obvious reasons,

is not designated by numerical values. The diagram will be better understood if one thinks of the human being as a human seeing-machine.

The lower curve may be regarded as a composite one representing numerous researches. As the intensity of illumination is increased, or as seeing is improved by other means, the production of useful work (and the energy expended in

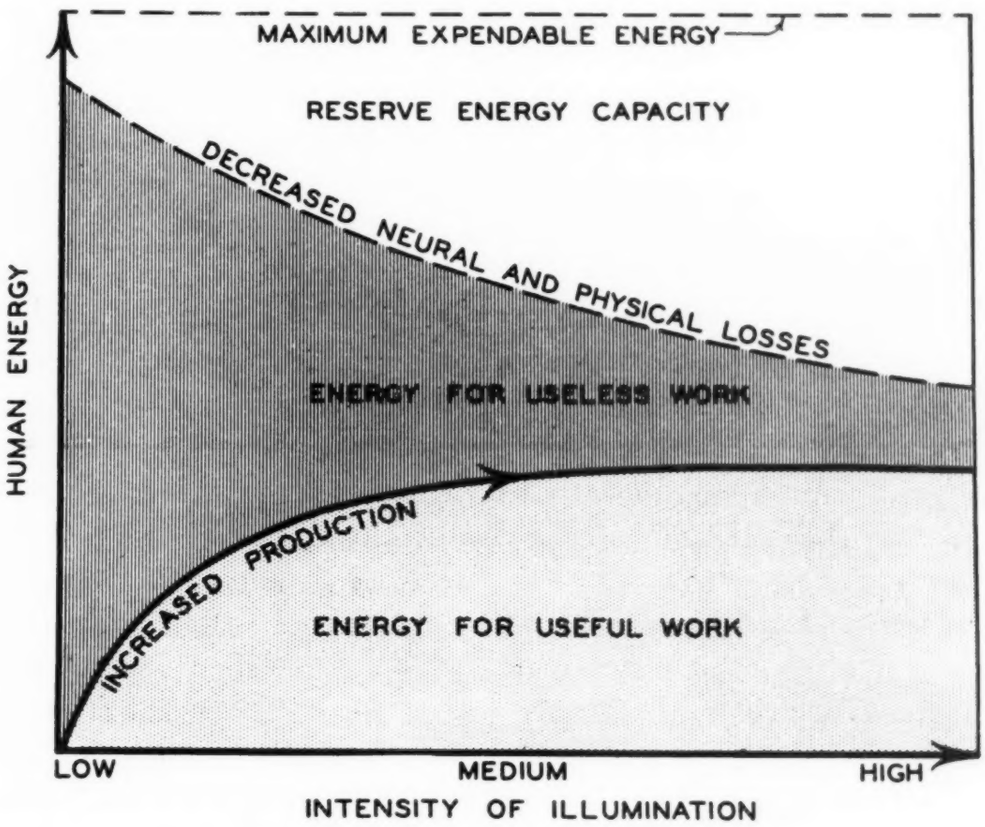

FIG. 1. A diagrammatic representation of the expenditure of energy by a human being operating as a seeing-machine. Quantitative data are presented in this discussion which pertain to the neural and physical energy losses resulting from inadequate seeing conditions. This phase of the subject is represented by the broken-line curve. The horizontal scale could also represent any other factors which influence seeing.

useful work) increases and eventually reaches an optimum, as shown. The form of this curve has been well established by many diversified investigations, the details of which are presented elsewhere (**3**). It is axiomatic that human beings also expend energy in performing useless work, which is analogous to friction losses in mechanical devices. Therefore, the total energy expended in performing a given task may be

considered to be the sum of these two components and is indicated by the upper broken-line curve. Since it has been established that higher intensities of illumination enable visual tasks to be performed more quickly and more precisely (4), it may be expected that higher levels of illumination are also capable of reducing the internal (psycho-physiological) human energy losses. The trend of the broken-line curve was originally deduced from this assumption. It was the purpose of the research, the results of which are discussed in this paper, to investigate the validity of this assumption. Specifically, this research sought to discover a possible correlation between intensity of illumination and human energy losses incident to reading.

Experimental Criterion

It is a matter of everyday experience that nervous tension and fatigue commonly cause a tightening of certain muscles. For example, a pencil may be held more tightly when one is nervous or after the muscles of the hand or arm have become fatigued from a long period of writing. In the latter case, the effect of the fatigued muscles is indicated by the reaction of those muscles. It may also be observed that muscular energy is often wasted as a result of strain or fatigue which originates remotely from the place where it may manifest itself. An automobile driver commonly grips the steering-wheel more tightly at night on a poorly lighted street than on a well-lighted one. In this case, an inadequacy in conditions for seeing is manifested in the useless expenditure of muscular energy. The importance of nervous tension in general fatigue of human beings is likewise obvious.

The muscular strain, using the term in its usual sense, produced by fatigue or strain is referred to as *nervous muscular tension* in the present discussion. Measurements of this factor are designated as *indicated nervous muscular tension*, since we have no means of knowing the completeness or the accuracy with which this criterion appraises or measures these internal psycho-physiological reactions. Obviously, any criterion selected to appraise the amount of human energy wasted in useless work is to be regarded as a relative rather than as an

absolute method of appraisal. Therefore, the value or usefulness of such a criterion corresponds to the directness with which it may be interpreted. Nervous muscular tension is to be regarded as an arbitrary, convenient and useful 'measuring-stick' for appraising complex psycho-physiological reactions to light, lighting, visual tasks and various conditions for seeing.

Experimental Variable

Intensity of illumination was selected as the experimental variable and all extraneous factors eliminated or reduced to a minimum. Tests were conducted at each of three intensities of illumination—1, 10 and 100 footcandles, respectively.

Procedure

The utilization of nervous muscular tension as a criterion for the appraisal of energy expended in useless work permits a wide choice of methods for its measurement. However, there are certain fundamental restrictions which are to be observed. It is obvious that the true purpose of the research should be concealed from the subjects in order that their reactions might not be altered by a consciousness of the objectives of the experiment. Hence, this requirement practically eliminates the possibility of attaching apparatus directly to the body of the subject in order to measure muscular tension. The method evolved and used in the research described in this paper consisted in measuring the changes in muscular tension as registered by the fingers of the left hand which rested naturally and lightly upon the knob of a concealed key.

The subjects were seated comfortably at a large table fastened securely to the floor, as illustrated in Fig. 2. Postures of subjects while reading were extensively studied as a preliminary phase of the research in order to arrange the experimental conditions for (1) maximum comfort and naturalness for the readers and (2) elimination of the influence of changes in posture upon the pressure unconsciously exerted upon the key. The height of the knob of the key above the table-top, the location of the key upon the table and the posi-

tion of the book were determined by trial. The forearm and elbow of the left hand were provided with special adjustable supports which allowed the fingers (middle and index) of the left hand to rest lightly and comfortably upon the key regardless of the posture assumed by the reader. Hence, a possible slight tendency on the part of the reader to lean forward and closer to the book at the lower levels of illumination did not

FIG. 2. Showing the position of subjects while reading, the location of the book and the operation of the key by means of which measurements of muscular tension in the fingers of the left hand were made.

directly transmit additional pressure to the key since the hand, forearm and elbow were supported by the table and a special arm-rest. Since the readers were instructed to assume the most comfortable posture at the beginning of each test, each posture was entirely independent of preceding test-conditions. The influence of posture is discussed in detail later.

The subjects were instructed to read at their *natural rate* during the thirty minute test-period, and cautioned against considering the problem as a 'speed-test.' In addition, they

were instructed to rest the index and second fingers of the left hand upon the knob of the key and to press this key firmly down as they read the last word on each page. This manual task served to justify the presence of the key and at the same time concealed the real purpose of the key. Even at the conclusion of the research, which was in continual operation for four months, none of the fourteen subjects, who completed the entire series, was aware of the real purpose of the research.

During each thirty-minute test-period, the reader was alone in a room separate from the experimenter and recording apparatus. Hence, he was not disturbed or influenced by the recording of data, the adjustment of apparatus, noise or other distractions. The apparatus which automatically recorded the variations in pressure upon the key was placed in sound-proof cases even though it was located in a room adjacent to the one occupied by the subject. The reading was performed steadily, quietly and continuously throughout the test-period, excepting for a rest-period of a half-minute inserted at the middle of the test-period.

Reading was selected as the visual task because of its obvious advantages as a universal, uniform and desirable visual problem. The book chosen was "Why We Behave Like Human Beings" by Dorsey. The reading matter consisted of 12-point Bodoni book-type printed well on excellent book-paper. Visually, this task was about as easy as reading can be. Each subject began reading at the beginning of the book and progressed steadily through it. As the subjects were interested in the contents of the book, attention was drawn from the key and natural reactions were expected.

The pressure unconsciously exerted upon the key was automatically recorded by a series of 10 electrical counting devices operating at intervals of 5 seconds throughout the test-period of 30 minutes. These counters were operated by the specially constructed key (Fig. 3) which was entirely concealed, excepting the knob. The index and second fingers of the left hand of the subject rested upon this exposed knob. Mechanical details of this key are presented later. It was established by preliminary tests that a range from 25 to 105

grams was sufficient to include nearly all pressures exerted by various subjects under various experimental conditions. These preliminary tests also indicated the probable distribution of the pressures to be recorded; hence, it was possible to

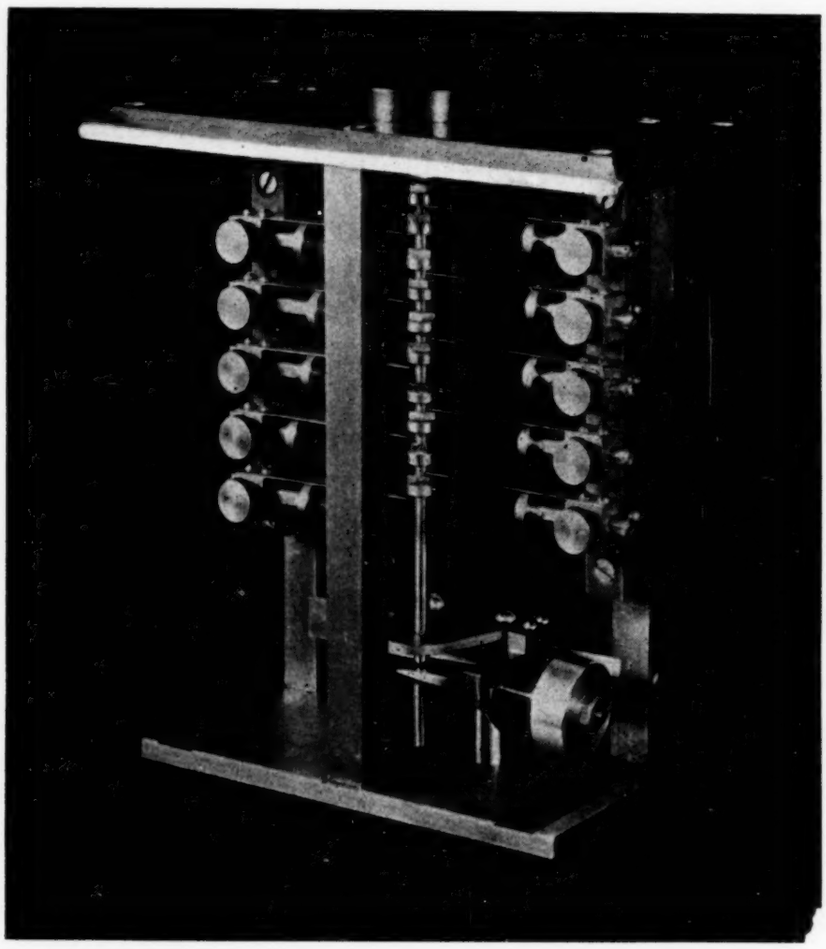

FIG. 3. Illustrating the mechanical construction of the key which responds to nervous muscular tension transmitted by the fingers of the left hand.

approximately adjust the 10 contacts of the key to correspond to this distribution.

DESCRIPTION OF APPARATUS

It is obvious that the quantitative relationship between indicated nervous muscular tension and intensity of illumination, as obtained by the present research, depends largely upon the physical characteristics of the key shown in Fig. 3. This key was so constructed that the maximum change in pressure unconsciously exerted by the subjects depressed the

knob of the key less than one-sixteenth of an inch. This change in the height of the knob of the key above the table-top was apparently so slight that it escaped notice by the subjects. The central shaft of the key (Fig. 3) rested upon a counter-balanced steel beam, which in turn rested upon knife-edge bearings. Ten accurately constructed brass discs were threaded upon the central shaft and held in place by lock-nuts. This arrangement provided a means for adjusting the key to close electrical contacts at certain pre-determined pressures. Phosphor-bronze cantilever springs were so arranged that 10 electrical circuits were successively closed for pressures varying from 25 to 105 grams. The effective length of the springs was alterable by adjustable supports and provided a simple means for calibrating the key. Friction losses were of the order of 1 gram. The key was extremely rigid in construction, as will be noted from Fig. 3. The upper and lower plates consisted of thick steel and were connected by three rods of one-half-inch cold-rolled steel fitted into milled grooves. The constancy with which the key maintained its calibration is indicated by the fact that calibrations made at intervals over a period of five months seldom varied more than plus or minus one gram.

The table and book were illuminated by light-sources located on the ceiling and directly over the subject and, since the book paper provided a matte surface, preventable glare was reduced to a negligible quantity. In addition, the inclined surface holding the book was covered with a medium gray cardboard for the purpose of reducing the brightness-contrast between the book and the immediate surroundings. The general surroundings were illuminated independently of the work-place in such a way as to eliminate high brightness-contrasts between the immediate surroundings and the general surroundings. The brightness of the latter was adjusted to approximately one-fourth the brightness of the pages of the book at each level of illumination.

Extent of Investigation

Fourteen adult subjects, 8 men and 6 women, of normal or nearly normal vision completed the entire series. Six 30-

minute tests on different days were made for each subject at the one-footcandle level of illumination. Eight tests on different days were made for each subject at levels of illumination of 10 and 100 footcandles, respectively. These tests were presented in haphazard order over a period of four months. This period, of course, does not include preliminary tests and investigations incidental to the main research.

Results

The experimental results and the corresponding relative probable errors are presented in Table I. During each 30-minute period of reading, 360 measurements of the pressure

TABLE I

The Relationship Between Indicated Nervous Muscular Tension and Intensity of Illumination; also a Statistical Analysis of the Reliability of These Data

The relative probable errors are derived in each case from the separate serial values. The relative probable error of the mean E_m is derived from the fourteen corresponding individual values E, by the formula $E_m = 1/N\sqrt{\Sigma E^2}$, in which $N = 14$. The variations due to the systematic differences between the subjects are, therefore, not included. Relative probable error is defined as the probable error of the mean divided by the mean.

Subject	Indicated Nervous Muscular Tension (Grams pressure unconsciously exerted on key) E is individual probable error in percent					
	1 Footcandle		10 Footcandles		100 Footcandles	
	Pressure (Grams)	E (Percent)	Pressure (Grams)	E (Percent)	Pressure (Grams)	E (Percent)
1	55.4	1.8	44.0	1.6	38.0	2.4
2	48.8	2.6	46.6	2.7	34.9	2.1
3	54.7	2.2	46.4	3.0	31.8	1.9
4	53.9	1.5	43.5	2.6	31.7	1.9
5	51.7	2.7	42.0	2.1	36.2	2.0
6	49.8	2.5	44.6	1.5	35.6	1.9
7	89.4	2.5	81.7	3.1	71.6	3.0
8	66.4	0.9	53.1	2.0	48.9	3.4
9	80.6	3.3	74.6	2.1	63.4	4.6
10	73.8	3.1	63.9	2.8	51.9	3.8
11	59.9	1.8	49.1	3.0	34.2	2.1
12	73.3	4.6	69.1	2.3	59.3	2.2
13	84.4	2.7	59.9	3.6	43.5	2.8
14	59.9	2.5	56.0	3.0	42.8	5.3
Mean	63.2*	0.7	54.1	0.7	43.0	0.8

* Geometric mean of the individual results.

exerted upon the key were automatically recorded. The averages of these series of measurements are presented in the second, fourth and sixth columns of Table I. There was a total of 308 test-periods of 30 minutes' duration, each involving 360 measurements of pressure. Of the 308 tests, the results of 9 tests were excluded from the calculations in accordance with Chauvenet's criterion. It will be noted from the summaries of these data that the *indicated nervous muscular tension*, developed incidental to reading, decreased from 63.2 grams to 43.0 grams as the intensity of illumination was increased from 1 to 100 footcandles.

The relative probable errors of the experimental results are

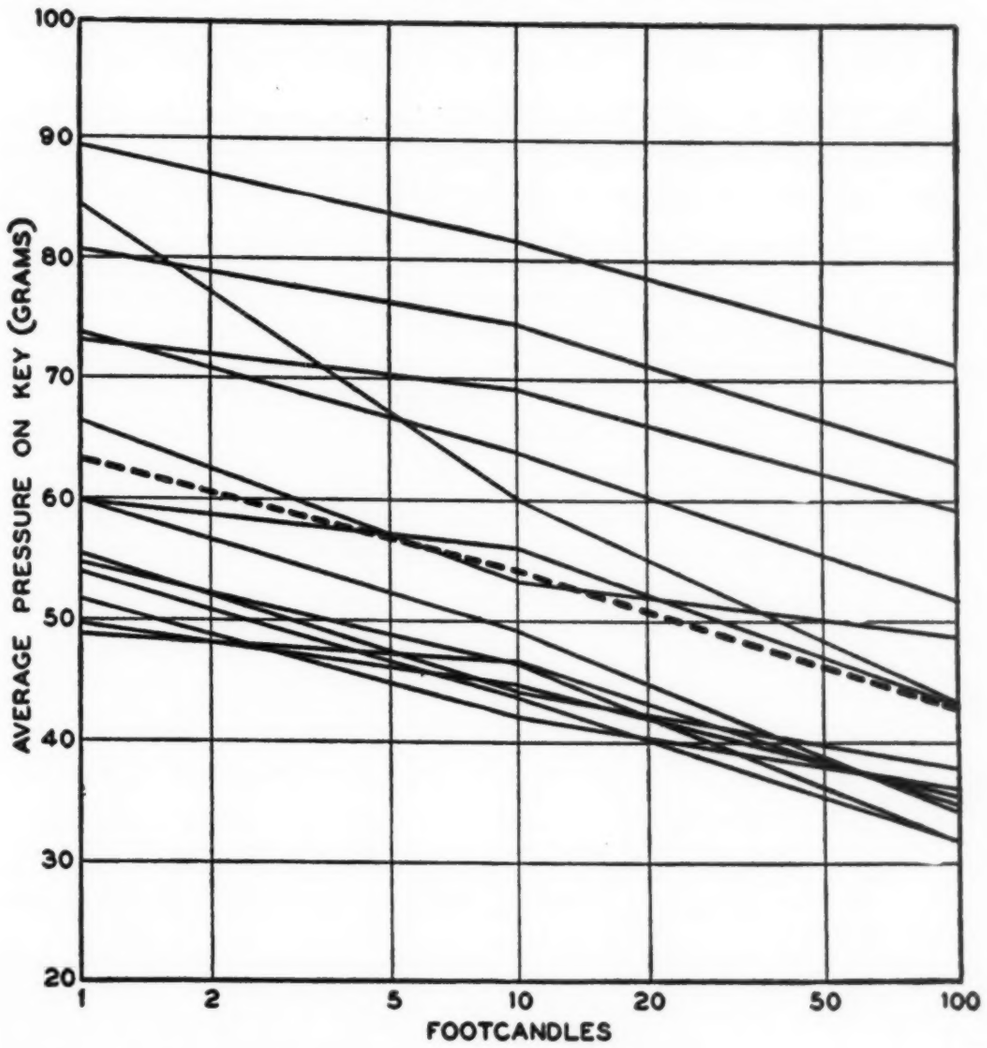

FIG. 4. The relationships between indicated nervous muscular tension and intensity of illumination for each of the fourteen subjects. The broken line represents the geometrical mean of the results of the fourteen subjects.

presented in the third, fifth and seventh columns of Table I. It will be noted that the probable errors of the average values of *indicated* nervous muscular tension are of the order of 1 percent; therefore, from the statistical viewpoint, these data possess a high degree of reliability. It will also be noted from Fig. 4 that each subject yielded similar results; that is, the pressure unconsciously exerted upon the key definitely decreased as the intensity of illumination was increased in each case. Such agreement among different subjects is not always obtained in researches upon human behavior. The consistency among the results for the several tests upon the same subject, as indicated by the individual probable errors, is additional evidence bearing upon the reliability of these data and upon the value of this method in studying the effects of conditions which influence seeing.

The rate of reading, in pages per minute, is given in Table II for the various subjects. Since the subjects were carefully

TABLE II

THE EFFECT OF INTENSITY OF ILLUMINATION UPON RATE OF READING UNDER THE CONDITIONS SPECIFIED IN THIS RESEARCH

Subject	Pages Read per Minute		
	1 Footcandle	10 Footcandles	100 Footcandles
1	.476	.569	.533
2	.594	.644	.637
3	.687	.724	.746
4	.631	.614	.622
5	1.346	1.265	1.446
6	.920	.867	.882
7	.762	.812	.800
8	.691	.742	.802
9	.701	.729	.708
10	.893	.810	.799
11	.790	.817	.839
12	.531	.497	.531
13	.310	.348	.352
14	.698	.750	.759
Geometric mean	0.680	0.700	0.712

instructed to read at their natural rates, it is significant that the average rate of reading for the group increased consistently as the level of illumination was increased. That the increase in rate of reading is rather small may be interpreted

as indicating that the visual task was relatively easy; otherwise, a comparatively large increase would have been obtained (**3**).

Discussion

A preliminary experiment established the important fact that an increase in the pressure unconsciously exerted upon the key by the reader (indicated nervous muscular tension) is a result of unfavorable conditions for seeing and for comfort. Increases in pressure upon the key were obtained (1) by introducing into the visual field a glare-source of such a brightness as to cause definite discomfort to the reader and (2) by lowering the intensity of illumination until seeing became difficult and trying.

The changes in pressure upon the key are unquestionably a result of changes in intensity of illumination because preventable glare did not exist in the research discussed in this paper. The simplicity of the experimental procedure and the naturalness of the visual task do not even suggest variables other than intensity of illumination.

An analysis of the various phases of this research indicates that there was practically no difference in the position of a given subject for the high and low intensities of illumination. It has already been shown (1) that the left arm and hand were so mechanically supported that significant changes in pressure due to changes in posture could not occur and (2) that the subjects adjusted themselves for optimum comfort at the beginning of each test; hence, no differential in comfort or pressure upon the key should exist due to posture from one test to another. The improbability of effects of changes in posture is also indicated by the results of a previous research bearing directly upon the relationship between the natural reading distance and intensity of illumination. In this investigation, the subjects were seated at a table and instructed to read aloud a series of letters of extremely low visibility. While they were reading, the distance from the eyes to the center of the book was measured, although the subjects were not aware that such measurements were being made. The test was repeated 4 times on different days for each of three

levels of illumination—5, 10 and 20 footcandles, respectively. The results are presented in Table III.

TABLE III

THE RELATIONSHIP BETWEEN INTENSITY OF ILLUMINATION AND NORMAL READING DISTANCE

Subject	Distance from Eyes to Book (Inches)		
	5 Footcandles	10 Footcandles	20 Footcandles
1	15.7	15.8	15.5
2	14.9	15.1	15.1
3	15.3	15.6	16.2
4	16.4	16.7	16.5
5	14.8	15.1	15.0
6	15.5	15.7	15.7
7	15.0	15.4	15.7
8	15.0	15.0	15.3
9	16.6	16.8	17.1
10	15.0	15.5	15.5
11	15.1	15.3	15.6
12	14.5	14.5	14.6
13	14.7	14.7	14.6
Average	15.3	15.5	15.6

It is obvious from these data that there was only a slight tendency on the part of 13 adult subjects to bring the book closer to the eyes to compensate for lower intensities of illumination, even though the visual task was a severe one compared with that used in the research under discussion. Hence, the probability of the subjects leaning forward and closer to the easily read book in the nervous tension experiment is remote; and the likelihood that such a slight change in posture would produce large changes in pressure upon the key is even more remote. Also, an extensive similar investigation was made in which reading at the normal distance was replaced by a task which involved the recognition of a moving test-object (subtending an angle of 5 minutes at the eye) placed at a distance of 100 inches from the eyes. Results similar to those presented in Table I were obtained. It is obvious, in this case, that the visual advantage secured by a possible leaning toward the test-object is negligible; hence, pressure on the key is explainable upon a basis of nervous muscular tension. Finally, no tendency to lean forward at the lower

intensities of illumination was observed in the research under discussion. The visibility of the printed matter was high, even at the one-footcandle intensity. Visual acuity was not a critical factor, because the size of the critical details was many times the threshold size. Therefore, it must be concluded that the changes in pressure upon the key resulted from nervous muscular tension, which, in turn, was decreased when the intensity of illumination was increased.

It is apparent that the differences between psycho-physiological reactions resulting from reading under different levels of illumination are of such definiteness and magnitude that they are capable of creating muscular strains which are measurable. Human energy expended in nervous muscular tension, in this case, is wasted, since it accomplishes nothing useful. This *indication* of the waste of human energy due to inadequate levels of illumination may be considered as a conservative appraisal of the actual conditions. Certainly, it is measured at a point remote from its origin. Apparently it can indicate nothing else than an effect of greater ease in performing a relatively easy visual task under intensities greatly above the threshold requirements.

The average values of indicated nervous muscular tension (in grams) and intensities of illumination are plotted in the upper part of Fig. 5. It will be noted that the data may be represented approximately by a straight line, when the footcandle values are plotted on a logarithmic scale. This indicates that the relationship between nervous muscular tension and intensity of illumination is generally logarithmic in character. The same relationship also exists between intensity of illumination and the several fundamental variables which influence visibility (**4**). In the lower part of Fig. 5, the relationship between intensity of illumination and the contrast required for threshold visibility (of an object equivalent in size to the book-type) is presented for comparison. The similarity between visibility relationships obtained by direct measurement and psycho-physiological relationships appraised indirectly is obvious and very important in the development of the science of seeing.

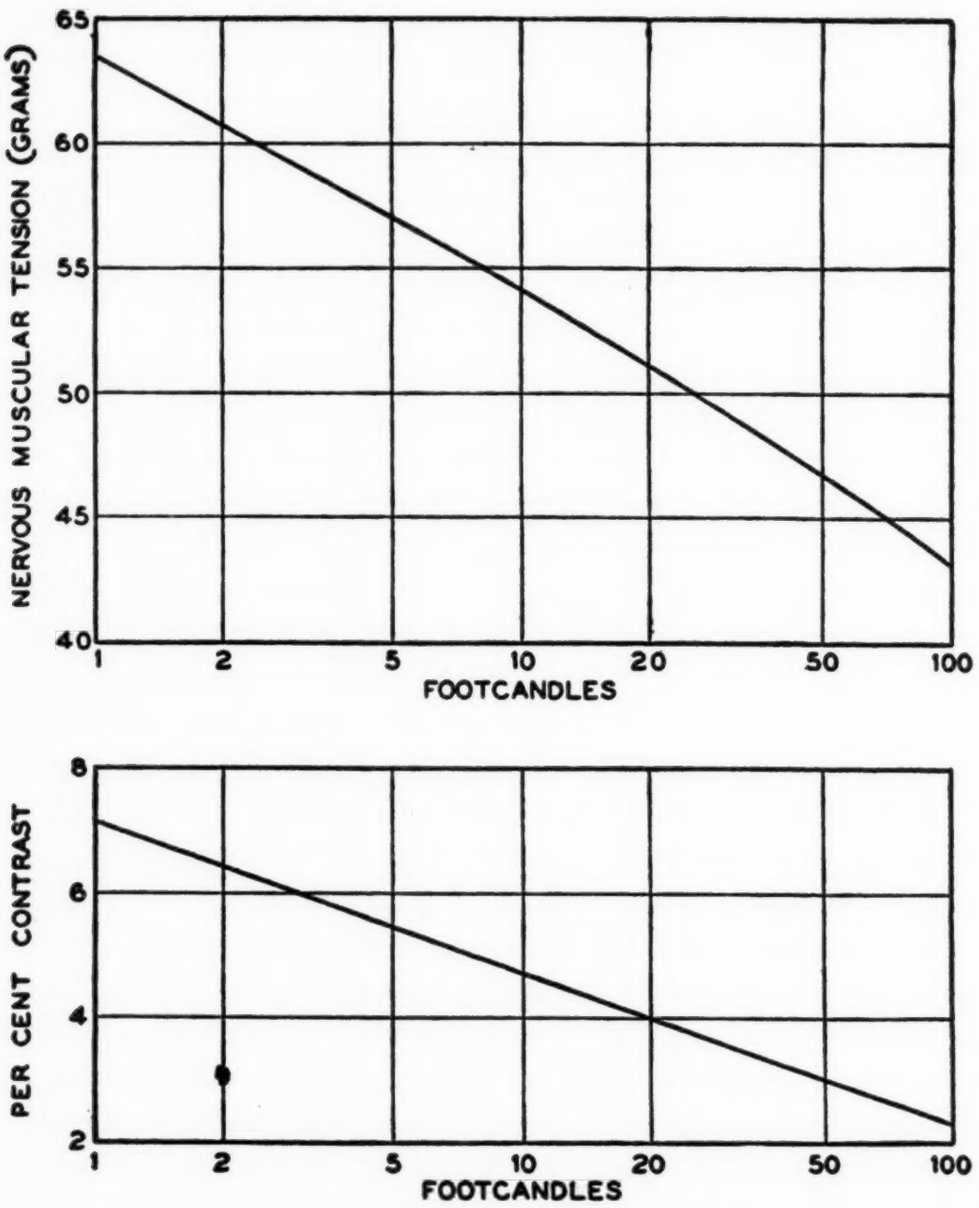

FIG. 5. The upper curve indicates the decrease in nervous muscular tension with increase in intensity of illumination.

The lower curve shows the relationship between contrast required for threshold seeing and intensity of illumination. It is one of the various examples of approximate logarithmic relationship of intensity of illumination and visibility factors.

It will be noted that both curves are approximately logarithmic in character.

The importance of these new data obtained in the psychophysiological realm may be readily seen by reference to Fig. 1. They may be regarded as data which seek to establish optimum conditions for seeing. They show how inadequate measurements of visibility are in appraising seeing conditions

for the human seeing-machine. They throw some light on the *ease* of seeing, whereas measurements of visibility and purely visual functions cannot be, directly at least, interpreted in this manner.

The work reported in this paper also serves to illustrate that a new science of seeing is evolving and that seeing is a super-complexity consisting of a chain of three complexities:

1. The external realm consisting chiefly of physical factors of object and environment.
2. The eyes and visual sense which, with incomplete relationships of external factors, have been the general objective of researches in physiologic optics.
3. The internal realm of psycho-physiological effects, which the research under discussion has invaded.

The final results or end-products of this chain of complexities which constitutes seeing are human behavior and welfare and the efficiency of human beings as human seeing-machines.

The authors acknowledge their indebtedness to Mr. S. K. Guth who took care of the details of this research during the entire seven months of preparation and operation.

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THE INFLUENCE OF TRAINING ON MOTOR TEST PERFORMANCE [1]

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Among fundamental problems in the field of applied psychology is that of developing tests of motor ability which can be used with a satisfactory degree of assurance in predicting the skill that can be acquired by an individual in manipulative activities. The practical importance of this problem has been well stated by Paterson and his co-workers at the University of Minnesota who point out than "in countries, like the United States, which have passed from an agricultural to an industrial state, a very increasing portion of the population is devoting itself to mechanical pursuits." [2] In this country nearly one-third of the working population, the largest single occupational class, is engaged in mechanical industries. If to these are added workers in other occupations requiring in part mechanical skill, it can be assumed that at least 40 percent of the gainfully employed population is dependent for its livelihood to some extent upon whatever underlies manipulative skill. In addition, in sport, which consumes so much of our leisure time, in ordinary everyday life, manual activities play a tremendously important rôle. The analysis of abilities underlying such activities represents therefore a problem of utmost practical significance.

In the analysis of motor ability, and in the development of tests for its measurement the investigator has been faced by a series of troublesome questions which must be answered before satisfactory batteries for forecasting in this field can be provided. Among these are such questions as:

[1] A paper read at the Fourth Spring Meeting, New York Branch, American Psychological Association, Yale University, April 1, 1933. The paper includes a few of the findings from a larger investigation, to be reported upon in greater detail, made possible by a grant from the Faculty Research Committee of the University of Pennsylvania.

[2] D. G. Paterson, R. M. Elliott, L. D. Anderson, H. A. Toops, E. Heidbreder, *Minnesota Mechanical Ability Tests*, 1930, p. 3.

"Is motor skill determined by a single, central factor that may be designated as mechanical ability?"

"Does it represent an ensemble of elementary functions united to constitute a 'gestalt' or 'pattern,' structually self-contained?"

"What is the relative influence of practice and personal aptitude in the constitution of skill?"

"To what extent can the underlying mechanisms of skill be improved by training?" [3]

The last problem, insofar as the use of tests in selection is concerned, has been well stated by Claparède, who points out that it has been assumed, by implication at least, that if an individual A exhibits an aptitude superior to B (on any type of test) he is also more capable of developing that aptitude by practice. In other words, the trainability (or educability) of an aptitude is supposed to be a sort of attribute of the aptitude and dependent upon its degree or quantity.[4] The use of scores obtained in the single application of a test, either of the analytic or analogous type, assumes that those "persons who make the highest scores will, on the whole, retain their high position after training." [5]

The practical implication of the problem of educability has been clearly pointed out by Myers who shows that: "In vocational guidance, on the one hand, we apply tests of general and of other mental ability, and tests of motor and mechanical ability, on the assumption that those persons who make the highest scores will on the whole retain their high position after training (in one or other special ability). We have no adequate evidence at present of the general truth of this assumption. In vocational selection, on the other hand, we estimate the value of tests of general and special abilities for any given occupation by applying them to persons who have already been trained in that occupation. Here we make the converse assumption that those persons who make the highest

[3] A. Gemelli, Recherches sur la nature de l'habileté manuelle, *J. de Psych.*, 1929, **25**, 164–65.

[4] E. Claparède, *L'Orientation Professionelle*, Geneva, 1922, p. 73.

[5] C. S. Myers, On Educability, *Proceedings*, V-e Conference Internationale do Psychotechnique, Utrecht, 1928, p. 104.

scores after training (not in the tests but at their occupation) would have done so likewise if the tests had been applied before training. But here again we have no proof how far this assumption is justified." [6]

The experimental evidence on the influence of training, or of what has been called 'educability,' in performances involving motor ability is highly contradictory. Chapman,[7] Bieneman [8] and Argelander [9] find increasing convergence of learning curves with practice in typing, resulting from the greater improvement of those starting with low scores as compared with those who start with higher scores. Similar findings in the case of other motor activities are reported by Argelander,[10] Roels and Van Wijk,[11] Ehinger,[12] Gemelli,[13] and others. On the other hand, Hildebrandt,[14] Eschowitz,[15] and Kellner,[16] point to the absence of converging levels of performance with practice in tasks involving manual ability.

However, in spite of the contradictory character of experimental findings, there is reason for suspecting that, at least in the case of some motor tests and of other manual activities, initial scores, based on trials of short duration, will not satisfactorily predict the status of the individual after a period of training.[17] Insofar as this is the case, every test selected for

[6] Ibid.

[7] J. C. Chapman, The learning curve in typewriting, *Am. J. Psych.*, 1919, **26**, 252 ff.

[8] D. Beineman, *Recherches sur l'aptitude dactylographique en vue de l'orientation professionnelle*, Bur. Intern. do Trav., Geneva, 1923, pp. 47.

[9] A. Argelander, Beitrage Zur Psychologie der Übung, *Z. ang. Psych.*, 1921, **19**, 1–38; also *Z. ang. Psych.*, 1923, **21**, 225–58.

[10] A. Argelander, Zur Frage der Übungsfähigkeit, *Psychot. Z.*, 1928, **3**, 142–147.

[11] F. Roels and J. van Wijk, *De invloed der oefening op de oorspronkelijke indivedueele verschillen im arbeidspretaties*, Mededeelingen uit het psychologisch laboratorium der Rijksuniversiteit te Utrecht, 1924, **1**, 65 ff.

[12] G. Ehinger, Recherches sur la développement de l'habileté manuelle par la pratique d'un metier manuelle, *Arch. de Psychol.*, 1927, **80**, 299–319.

[13] A. Gemelli, Recherches sur le diagnostic de l'habileté motrice, *Rev. de la Sci. du Trav.*, 1929, **1**, 181–196.

[14] H. Hildebrandt, Übung und Eignungsprüfung, *Ind. Psychot.*, 1926, **3**, 96–109.

[15] N. Eschowitz, Zur Frage der Bewährung von Handgeschicklichkeitsproben, *Psychot. Z.*, 1927, **2**, 23–26.

[16] H. Kellner, Neun Jahre Prüferfahrungen in der Berliner Metallindustrie, *Ind. Psychot.*, 1928, **5**, 46–47.

[17] B. Kern, *Wirkungsformen der Übung*, Muenster, 1930, pp. 508.

use in measuring motor ability must be evaluated from the viewpoint of changes in relative levels of performance with practice and compared with long-time as well as short-time performance on the activity for which prediction is being made when the test is designed for use in forecasting proficiency in another manual task in industry or in the school.[18]

The problem of 'educability' on motor tests is being investigated as part of a larger study which includes in its scope an examination of work curves, the relation of feelings of monotony to the shape of the work curve, etc. The present report deals only with the procedure of this study and with a few of the findings that appear to be of significance in relation to the general problem of the effect of training upon tests of motor ability.

The subjects of the study were 46 boys and 50 girls ranging from approximately 15 to 18 years in age. The boys were, in the main, unemployed continuation school students. The girls included a group of students at the Philadelphia Trade School enrolled for training in dressmaking and allied trades. All of the girls had completed the 8th grade, and a number had gone as far as the second year in high school before transferring to the Philadelphia Trade School.

The tests employed included (1) the *Otis Self-Administering Test of Mental Ability* (Intermediate Form A), (2) *Stenquist Mechanical Assembly Test*, (3) *Minnesota Paper Formboard*, (4) *O'Connor Wiggly Block*, (5) *Serial Discrimeter* and (6) *Spool Packing Test* from the *Stanford Motor Skills Unit*, and the (7) *Roberts' Obstacle Test.* Most of these are sufficiently well known to require no description. The *Serial Discrimeter* [19] is a test of finger coördination in which the subject responds by pressing one of four keys with fingers 1, 2, 3, 4, respectively, in response to the numbers 1, 2, 3, 4, respectively, appearing in random order in a small opening on a circular disc faced by the subject.

The *Brown Spool Packing Test* consists of a frame holding a horizontal tray over a hopper. The hopper is filled with

[18] E. Claparède, *Op. cit.*, p. 8.

[19] See R. H. Seashore, Stanford Motor Skills Unit, *Psych. Monog.*, 1928, **39**, 51–65; Individual Differences in Motor Skills, *J. Gen. Psych.*, 1930, **3**, 38–66.

spools to be placed into the tray. The subject is required to use both hands and, immediately after filling the tray, to push it to the left or right and to start immediately to fill a second tray which appears when the first is pushed over. Twelve spools can be placed into the tray. It is a highly repetitive task involving bi-manual coördination.

The *Roberts' Obstacle Test*, an adaptation of a test described by Vickers and Hoskin,[20] measures speed of movement in a task calling for manual dexterity. A wooden block about 3″ long, 1½″ wide and ½″ thick must be passed quickly over and under a series of obstacles and through a series of slots each only large enough to permit the block to pass through, and each placed at a somewhat different angle on a vertical board.

The test period was divided into two sessions. During the first session the subject was given each of the seven tests. In this period the standard procedure followed by Seashore in giving the *Serial Discrimeter* and *Spool Packer* was employed. This consists of nine 2-minute trials on the *Serial Discrimeter* and three successive 3-minute trials on the *Spool Packer*. Trials on the two tests were alternated. In addition, the order of presentation of these tests was alternated from subject to subject.

During the second test session, occurring on the average 3 days after the first session, each subject was required to spend 2 hours on the *Serial Discremeter* and on the *Spool Packer*. The subject was placed alone in a room. Graphic records of performance were obtained on the Poppelreuter *Arbeitsschauuhr*.[21] For the purpose of this experiment an electrical connection was substituted for the mechanical connection on the original apparatus, making it possible to obtain records, without the subject's knowledge, in an adjoining room. The subject was instructed to work as fast as he could. He was also told that he could rest when tired. The

[20] W. Vickers and V. H. Hoskin, Results from some new tests of practical abilities, *Brit. J. Psych.*, 1929, **20**, 66–81.

[21] W. Poppelreuter, Die Arbeitskurve in der Diagnostik von Arbeitstypen, *Psychot. Z.*, 1928, **3**, 35–51; Die Arbeitskurve in der Eignungsprüfung, *Ind. Psychot.*, 1926, **3**, 161–67.

curves obtained show the time taken for each tray of 12 spools and for each set of 100 finger reactions on the serial discrimeter.[22]

A few of the findings that are of significance in answering the questions on the influence of 'educability' raised in the introduction to this discussion are shown in the accompanying tables. These findings are necessarily of a preliminary character, but they show trends which it seems of interest to present at this time.

1. Of particular interest are the *relatively* low coefficients of correlation between short and long spells of work in the case of both the *Serial Discrimeter* and the *Spool Packer* (Tables I and II). Correlation between 18 minutes (A) and a 2-hour run (B) (on another day) on the *Serial Discrimeter* yields a coefficient of .69 ± .04. In the case of the *Spool Packer* the coefficient of correlation between the standard 9-minute run (A) and one of 2 hours (B) is only 0.53 ± .05. Although both coefficients are high in comparison with the general run of aptitude forecasting coefficients, they are extremely low when it is recognized that we are dealing with the same activity on both axes of the correlation chart.

If a short run on simple tasks of this kind has so little predictive value in relation to long runs on these same tasks, the forecasting value for other forms of behavior can well be questioned. This finding, if borne out by further investigation, is evidence in support of the contentions by Poppelreuter, Kern and others (*a*) that initial scores on tests of short duration may lead to gross errors in the diagnosis of individual motor ability and (*b*) that all tests of motor ability must be carried to that critical point in the learning curve where stability of performance is attained.

2. On both the *Serial Discrimeter* and the *Spool Packer* the values of the coefficients consistently decrease as the short run is compared with successive half hour periods of the long run (Tables I and II). This evidence favors the point

[22] An interesting feature of these curves is a record of voluntary rest pauses which are being analyzed in the detailed study of work curves which represents one of the chief purposes of this investigation.

TABLE I

COMPARISON OF SHORT AND LONG TRIALS ON THE SERIAL DISCRIMETER
(84 Subjects—46 M and 38 F)

	r	P.E.
Serial Discrimeter A with Serial Discrimeter B, 1st half hour	.62	.05
Serial Discrimeter A with Serial Discrimeter B, 2d half hour	.56	.05
Serial Discrimeter A with Serial Discrimeter B, 3d half hour	.57	.05
Serial Discrimeter A with Serial Discrimeter B, 4th half hour	.61	.05
Serial Discrimeter A with Serial Discrimeter B, 1st hour	.61	.05
Serial Discrimeter A with Serial Discrimeter B, 2d hour	.62	.05
Serial Discrimeter A with Serial Discrimeter B, two hours	.69	.04
Serial Discrimeter (1st hour) with Serial Discrimeter (2d hour)	.85	.02
Serial Discrimeter (1st and 3d half hour) with Serial Discrimeter (2d and 4th half hour)	.85	.02

TABLE II

COMPARISON OF SHORT AND LONG TRIALS ON THE BROWH SPOOL PACKER
(84 Subjects—46 M and 38 F)

	r	P.E.
Spool Packer A with Spool Packer B, 1st half hour	.68	.04
Spool Packer A with Spool Packer B, 2d half hour	.51	.05
Spool Packer A with Spool Packer B, 3d half hour	.46	.06
Spool Packer A with Spool Packer B, 4th half hour	.34	.06
Spool Packer A with Spool Packer B, 1st hour	.66	.04
Spool Packer A with Spool Packer B, 2d hour	.42	.06
Spool Packer A with Spool Packer B, two hours	.53	.05
Spool Packer (1st hour) with spool Packer (2d hour)	.70	.04
Spool Packer (1st and 3d half hour) with Spool Packer (2d and 4th) half hour)	.76	.03

of view expressed by Van der Weldt,[23] Gemelli [24] and others that the entire pattern of an activity undergoes a change with practice. This finding is of particular interest in the light of what other studies have shown concerning changes in the continuity, speed and other characteristics of movement with practice.

3. Changes in the values of coefficients when short runs on the *Serial Discrimeter* and *Spool Packer* (A), as compared with long runs (B), are correlated with other tests, may constitute further evidence of such a change in pattern (Table III). Such variations are particularly noticeable in the case of the Obstacle Test and of the Stenquist Mechanical Assembly Test.

4. Correlation of performance during the first and second hours yield an r of $.85 \pm .02$ in the case of the *Serial Discrimeter* and of only $.70 \pm .04$ in the case of the *Spool Packing*.

[23] A. Van der Weldt, L'Apprentissage du mouvement et l'automatisme, *Études de Psychologie*, 1928, 3, 239 ff.

[24] See footnote 3.

Neither can be considered high in the light of the duration of the testing periods, as compared with the shorter period used in the standard application of the *Stanford Motor Skills Unit* and with those ordinarily employed in testing motor ability. This finding has also significance in the consideration of the question of changed patterns of behavior with increasing length of performance mentioned above.

5. The correlation of the first and third half hours with the second and fourth half hours of the long run yields a coefficient of .85 ± .02 in the case of the *Serial Discrimeter* and of .76 ± .03 in the case of the *Spool Packer*. Applying the Brown-Spearman Formula, this gives coefficients of reliability of .92 ± .01 for the two hour run on the *Serial Discrimeter* and of .80 ± .03 on the *Spool Packer*. These are very slightly lower than the coefficients of reliability on the shorter runs, reported by Seashore, which are .94 ± .02 and .86 ± .03 respectively.[25] The pragmatic significance of these is to be further investigated in a study involving a comparison of alternating odd and even five minute periods of performance.

6. Intercorrelations among the seven tests, both when the short and long runs on the *Serial Discrimeter* and *Spool Packer*

TABLE III

INTERCORRELATION OF TEST SCORES[1]

	Spool Packer A		Obstacle Test		Wiggly Block		Stenquist Mechanical Assembly Test		Minnesota Paper Formboard Test		Otis I.Q.	
	r	P.E.	*r*	P.E.	*r*	P.E.	*r*	P.E.	*r*	P.E.	*r*	P.E.
Serial Discrimeter A...	.43	.06	.32	.06	.00	.07	−.15	.07	.10	.07	.05	.07
Serial Discrimeter B...			.01	.07	−.10	.07	.35	.06	.02	.07	−.03	.07
Spool Packer A.....			.37	.06	.04	.07	−.18	.07	.12	.07	.04	.07
Spool Packer B.....			.09	.07	−.07	.07	−.22	.07	.07	.07	.07	.07
Obstacle Test.....					.11	.07	.13	.07	.14	.07	.19	.07
Wiggly Block.....							.46	.05	.36	.06	.33	.06
Stenquist Mechanical Assembly Test.....									.27	.06	.26	.06
Minnesota Paper Formboard Test.....											.44	.06

[1] *N* for Serial Discrimeter B and Spool Packer B = 84 (46 M and 38 F). *N* for all other tests and intercorrelations = 96 (46 M and 50 F).

[25] R. H. Seashore, *J. Gen. Psych.*, 1930, 3.

are considered, are varied in value, but generally low (Table III). Of 31 coefficients 20 are below .20 and only 3 above .40. The exact interrelationship among these, with respect to the possible existence of group determiners, remains to be studied.

The findings presented are only a few which have already appeared in the study under discussion. Of equal importance are the procedures of the study which, through the use of methods for obtaining graphic and detailed work curves, have yielded other significant findings to be presented in more complete detail and critically discussed in another article.

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THE FUNDAMENTAL PITCH OF ENGLISH VOWELS *

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Introduction

Through the work of Miller (**4**), Paget (**5**), Crandall (**3**), Liddell (**2**), Fletcher (**1**), and others who have followed Helmholtz, it is now known that the different vowel sounds are each characterized by, and probably recognized through, high-intensity overtones within a definite and fairly narrow frequency band. Liddell (**2**, III) has summarized these findings in his "vowel spectrum," which is reproduced in part in Table I, the vowels indicated by their sound in common English words.

TABLE I

Liddell's Frequency-Constants for Certain Vowels

Front Series		Back Series	
Vowel	Freq.	Vowel	Freq.
sot	900	suit	400
sat	1200	put	500
set	1400	sought	800
sit	1900		
seat	2100		

Liddell, in discussing such frequency constants, says, "It must be borne in mind, however, that these numbers are independent of the fundamental pitch of the vowel tones they characterize." (**2**, I, p. 16.) Other writers tend not to men-

* The writer is deeply indebted to Professor Edward Sapir of Yale University, in whose department the present work was done, for his personal interest and encouragement; and for his aid, as sponsor of this study, in developing the problem and interpreting the results. It is desired further to acknowledge indebtedness to Professor C. L. Hull, of Yale, for technical advice concerning set-up and statistical interpretation, and for the use of apparatus of his own design. Professor C. E. Parmenter and his staff, of Chicago, have very kindly read and criticized parts of the manuscript.

tion fundamental [1] pitch at all; and the general feeling implied is that one vowel, separated from a meaningful context, should naturally be pronounced at the same pitch as any other vowel. It is quite obvious that any constant natural differences of fundamental pitch between the vowels would be almost impossible to observe in actual speech, because of the superposition of melody and emphasis factors. Nevertheless, certain rather generally accepted principles lead one to believe that other things being equal, the natural fundamental pitch of the different vowels should vary in a quite definite and specifiable manner.

There may be considered, in the first place, the phenomenon of dynamogenesis, or irradiation of tension from one set of contracted muscles to another set of muscles. In conjunction with this, there may be mentioned the generally accepted view of phoneticians regarding the position of the tongue in speaking the various vowel sounds. Starting with the vowel in 'sot,' in speaking which the tongue lies fairly relaxed and low, the sounds in 'sat,' 'set,' 'sit,' and 'seat,' represent progressive rising of the dorsal part of the tongue—presumably with increased tension—toward the pre-palatal and palatal regions. In other words, the tongue moves progressively up and forward in the mouth in speaking this series of so-called 'front' vowels.[2] Again, starting with 'sot,' the dorsal and post-dorsal parts of the tongue move progressively up and back toward the post-palatal region in speaking the series, 'sought,' 'put,' and 'suit,' which are called 'back' vowels. This relationship is often represented by the so-called vowel triangle, successive positions of which represent the position of the tongue relative to "sot" as a base. The vowel triangle is usually represented substantially as follows:

[1] By 'fundamental' pitch, the writer will consistently mean 'laryngeal' or 'cord' pitch, which may be freely varied in speaking or singing. This is to be contrasted with 'oral' pitch, which gives the vowel-quality, and to which Liddell's spectrum refers.

[2] The vowel in 'sot' is throughout this article classed as a front vowel, though it is actually at the point of the 'triangle.' It may be mentioned also that by no means all the vowel-sounds in the English language were used in this experiment. All diphthongs and 'mixed' vowels were omitted; but it will be fairly obvious that such omissions have not substantially altered the character of the results obtained. Furthermore, the vowel in 'suit,' historically a diphthong, was given the more current pronunciation as in 'boot,' by every subject used in this study.

Front Vowels	*Back Vowels*
seat	suit
sit	
	put
set	
	sought
sat	
sot	

In spite of some recent contradictory evidence (**6**, **7**), this schema of the tongue-positions may yet be accepted with some assurance. The third principle quite generally accepted is that a rise of cord pitch is brought about by progressive tensing of certain parts of the cord musculature, mainly the cricothyroid and thyroarytenoid muscles.

These three points, then, led by implication to the following hypothesis: if the vowel triangle is a true picture of progressive tensing of the tongue musculature; if progressive tension of the cord musculature raises the fundamental—cordal or laryngeal—pitch of the sound emitted; and if dynamogenesis is here operative; then the series 'sot,' 'sat,' 'set,' 'sit,' 'seat,' should show a progressive rise in pitch, and the series 'sought,' 'put,' 'suit' should also show a progressive rise in pitch. The following experiments were performed to test this hypothesis.

Apparatus

A horizontal smoked-paper kymograph was used throughout the experiment. The drum was 12″ long and approximately 6″ in diameter. It was driven by an 1800 R.P.M. synchronous motor with a worm reduction giving a drum speed of 90 R.P.M. The surface speed of the paper was 28.33 inches per second.

The standard supporting the voice tambour was mounted to slide on a track parallel to the drum. A screw, extending through a hole in the tambour standard, was mounted parallel to this track. The screw was driven by a belt connecting it with the drum shaft. Through the manipulation of a simple clutch, the experimenter could engage the tambour support to this screw in such a way as to move the former along its track at a uniform rate of approximately 1/6″ per revolution of the drum. This movement could be discontinued and resumed at any point on the drum as the experiment should demand. With this apparatus it was possible, on a single 12″ sheet of paper, to obtain over 100 feet of tracings, representing at least 44 seconds of continuous speech.

The tambours were of a very sensitive type developed by Professor C. L. Hull,

and described in Weaver's study (8). For the capsule a 5/16″ hole was drilled in a piece of brass rod ½″ in diameter. The recording lever was a flexible, tapering glass needle drawn out from small glass tubing to a diameter of about .17 mm. This needle was attached to the drum-head by means of a triangular base of glue. The appearance of the tambour is shown in Figure 1. The vibration intake for the tambour was a long piece of copper tubing. The mouthpiece, which was a small funnel cut away to fit snugly around the subject's mouth, was connected to the copper intake by a short section of rubber tubing. This rubber tubing was kept as short as possible so as to reduce the loss of energy through absorption by the rubber.

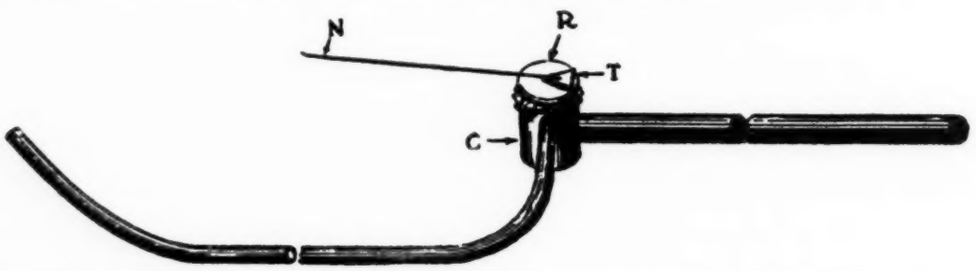

FIG. 1. Hull's tambour for voice recording. *C*=capsule. *R*=rubber tambour head. *T*=triangular base of glue. *N*=tapering glass recording needle. Vibration intake is through curved copper tube.

THE PITCH OF THE VOWELS, MEN SUBJECTS

For this study, five front vowels were used in the words, 'sot,' 'sat,' 'set,' 'sit,' and 'seat,' and three back vowels in 'sought,' 'put,' and 'suit.' They were typed on a card in twenty-six numbered pairs, as shown in Table II. It will be seen that each vowel of the front series is paired twice with each of the other four of that series, as 'sot—sat,' and 'sat—sot,' and that the resulting twenty pairs are arranged in random order. The three back vowels are paired and arranged in similar fashion. Positional effects within each series seem to be fairly well cancelled out.

The subjects were eight undergraduates of Yale University. Verbal instruction was given each of them substantially as follows:

I wish to make a record of your voice as you say the following pairs of words. I shall say "Ready for the first pair . . . go," and then you will say the first pair, separating the two words enough so that you feel you are through saying the first one before you start saying the second. Then we shall pause a moment and I shall say "Ready for the second pair . . . go"; and you will say the second pair, and so on. You will speak through this mouthpiece, which you will have to hold quite snugly against your face. It may seem awkward to you at first, but we shall practice until you feel fairly comfortable with it. Will you try the first pair now, for practice, with our usual signal?

Then practice was given on the first pair, without recording, until the subject announced that he knew how to do it. Practice was then continued with recording, and the subject was asked, if necessary, to hold the mouthpiece more snugly or occasionally to speak a trifle more loudly, until the voice tracing was of readable amplitude. After this practice, the experimental series of pairs listed in Table II was recorded with

TABLE II

THE EXPERIMENTAL SERIES OF PAIRED WORDS

1. sot—sat	14. sit—sot
2. set—sit	15. seat–sat
3. sat—seat	16. sit—sat
4. sot—set	17. seat—sot
5. seat—set	18. set—sat
6. sat—set	19. sit—set
7. seat—sit	20. sat—sot
8. sot—sit	21. suit—put
9. set—seat	22. sought—suit
10. set—sot	23. put—suit
11. sat—sit	24. sought—put
12. sot—seat	25. put—sought
13. sit—seat	26. suit—sought

little delay. Pairs of words were repeated whenever the experimenter wished, when recording was poor or when the subject coughed or had any other trouble. Never at any time did the experimenter say any of the words, or indicate the purpose of the experiment.

It will be noted from the list of words spoken by the subject that there are eight repetitions of each front vowel, and four repetitions of each back vowel. The pitches of all repetitions —eight or four—of a given vowel were averaged for each subject. These final pitch values of each vowel for each subject are presented in Table III, as well as the mean pitch of each vowel for all subjects combined; the difference between the mean pitches of successive vowels on the traditional triangle; the probable error of that difference, its critical ratio, and the consequent statistical chances that there is a true difference greater than zero. It will be seen that every one of the differences is in the direction deduced from our basic hypothesis; and while not all of the critical ratios are large enough to

TABLE III

PITCH OF THE VOWELS, MEN SUBJECTS

Subj.	Front Vowels					Back Vowels		
	sot	sat	set	sit	seat	sought	put	suit
EEG..	180.5	176.6	186.1	199.2	200.0	185.6	188.0	199.2
JHL...	109.9	104.5	110.6	119.0	118.7	105.4	109.7	109.8
KvS...	113.7	116.4	121.4	121.5	129.9	—	—	—
BCH..	139.2	143.4	144.1	153.2	153.6	143.5	169.5	161.3
IG.....	113.4	117.4	123.7	133.1	129.5	117.8	124.8	125.5
JJC....	123.9	124.9	128.5	145.8	139.8	135.8	149.9	155.2
DB....	143.9	144.6	149.1	160.7	162.0	125.2	152.4	145.8
EHM..	134.5	138.8	141.3	147.9	162.1	140.3	159.1	169.0
Mean..	132.38	133.33	138.10	147.55	149.45	136.23	150.49	152.26
Difference...	0.95	4.77	9.45	1.90		14.26	1.77	
PE diff.....	0.924	0.659	1.12	1.43		2.25	1.70	
Diff./PE diff.	1.03	7.25	8.40	1.32		6.32	1.04	
Prob. that true diff. >0.....	0.76	0.999+	0.999+	0.82		0.999+	0.76	

lend statistical certainty, the regularity of the progression and its conformity to widely-held principles give considerable weight to a belief in the reality of those differences.

The final mean pitch values of the front and back series of vowels are shown graphically in Fig. 2. A glance at this figure shows that there has emerged from our experiment a new vowel triangle exactly paralleling the traditional triangle which was based on functional anatomical considerations and had no pitch significance whatever. This new triangle, however, is based purely on the natural fundamental-pitch relationships of the vowels. Obviously the actual pitches found in this study are peculiar to the particular subjects used; but it seems quite justifiable to generalize the form of this new figure, and, in this broad sense to speak of it as the 'vowel-pitch triangle.'

THE PITCH OF THE VOWELS, WOMEN SUBJECTS

The entire procedure detailed above was repeated, using nine young women of college age as subjects. Pairs 21–26

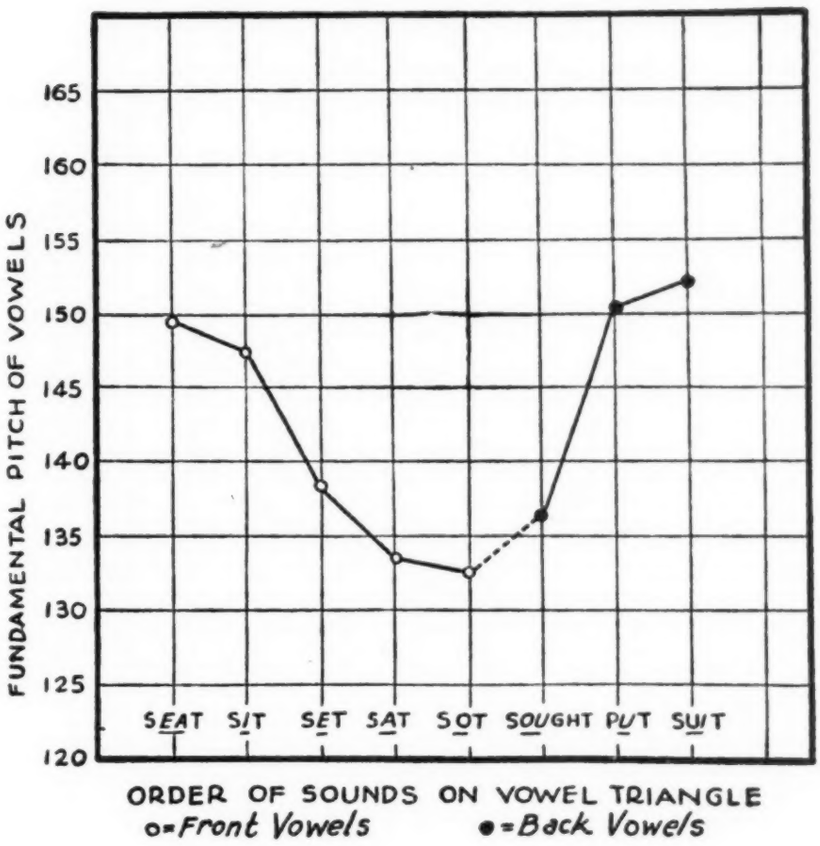

FIG. 2. The vowel-pitch triangle derived from data on men subjects. The vowels are placed on the abscissa in the order required by the traditional 'vowel-triangle' based on anatomical considerations. The ordinates, showing the fundamental pitch of those vowels when spoken in isolation from a speech-context, form a pitch-triangle.

of the series shown in Table II, which involve the three back vowels, were repeated twice by these women, thus giving eight repetitions of every vowel, both front and back, by each subject. Table IV, which is comparable to Table III, reveals that once again, every difference is in the direction required by our hypothesis; and to the previous fairly adequate proof of that hypothesis, we may now add its substantiation in a second and entirely independent series of measurements made on a different type of voice. Figure 3 presents the final mean pitch values of all the vowels as spoken by the women subjects. Its striking similarity to Figure 2 leaves little doubt that the vowel-pitch triangle represents a genuine characteristic of speech behavior.

TABLE IV

PITCH OF THE VOWELS, WOMEN SUBJECTS

Subj.	Front Vowels					Back Vowels		
	sot	sat	set	sit	seat	sought	put	suit
AW....	290.5	291.0	286.2	292.4	306.1	311.2	310.5	320.0
JT.....	394.7	393.9	406.1	402.2	408.6	377.8	382.6	400.4
SR....	260.3	267.7	272.4	277.8	279.0	255.6	270.2	280.2
GS....	325.9	329.4	332.2	355.6	363.8	329.4	344.8	387.4
EF....	262.9	265.5	262.9	277.0	280.7	275.1	287.2	283.4
MW...	226.0	232.6	228.9	252.9	247.6	240.6	245.6	257.1
EG....	285.2	290.8	298.1	331.2	324.5	296.2	307.7	307.9
SG....	380.7	380.0	385.3	402.5	393.9	397.6	419.4	415.0
DC....	258.3	262.6	274.1	271.9	275.9	239.7	241.7	254.2
Mean..	298.3	301.5	305.1	318.2	320.0	302.6	312.2	322.8
Difference...	3.2		3.6	13.1	1.8	9.6		10.6
PE diff.....	0.514		1.31	2.66	1.62	1.53		3.03
Diff./PE diff.	6.22		2.74	4.91	1.11	6.25		3.50
Prob. that true diff. >0.....	0.999+		0.96	0.999+	0.77	0.999+		0.99

DYNAMOGENESIS AS AN EXPLANATORY FACTOR

While the facts so far discovered were deduced from a set of principles involving dynamogenesis, it is entirely possible that such irradiation as was postulated does not as a matter of fact occur. If dynamogenesis is operative as assumed, then it should be possible to reproduce the phenomenon experimentally in some manner independent of the previous set-up. For example, if *tongue* tension raises vocal pitch through irradiation, then *hand* tension should produce a similar effect. To test this deduction, the following experiment was performed using fifteen men as subjects. Each subject was asked to hold a hand-dynamometer in one hand, and was given the following instructions: "Now we shall use only the word 'sat.' Sometimes I shall ask you to say that word while squeezing the dynamometer. The signal will be, 'Now with squeezing. Ready . . . go,' or, 'Now without squeezing. Ready . . . go.'" Calling 'no-squeeze' 'a' and 'squeeze' 'b,' the formula for the proper order is a-b-b-a-b-a-a-b alter-

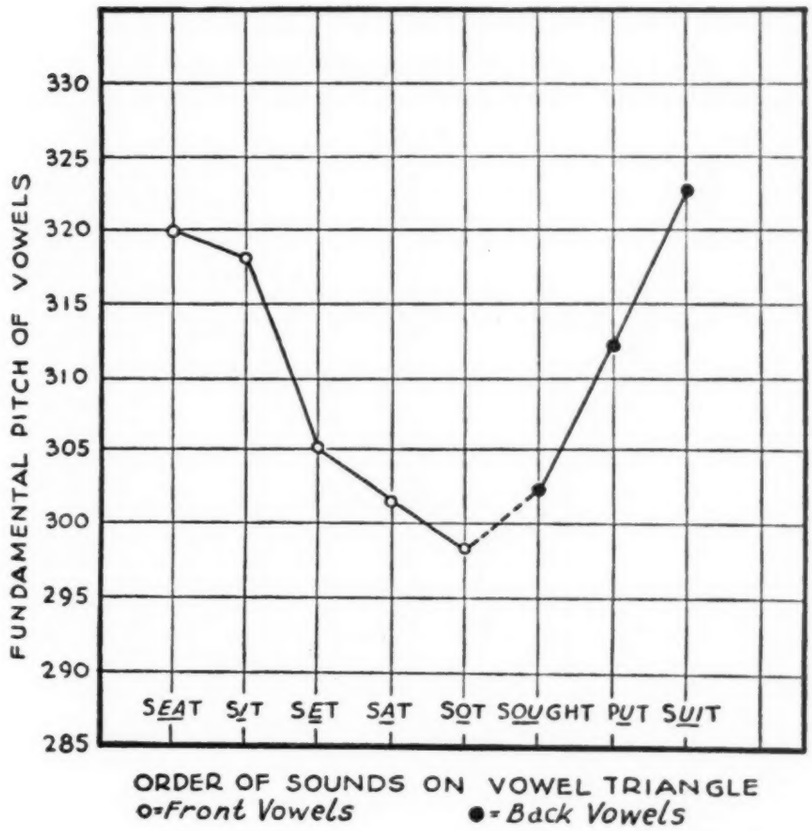

FIG. 3. The vowel-pitch triangle derived from data on nine women subjects. This represents an independent repetition of the procedure from which Figure 2 was derived.

nated with b-a-a-b-a-b-b-a. Three of the subjects were recorded on only one section of this, giving 4 values for each of conditions 'a' and 'b.' Seven were recorded with both sections, giving 8 values for each condition; and for four subjects, 12 values on each condition were secured.[3]

Table V shows the results, which were treated as described in preceding sections. It is clear from this table that the experiment has yielded results entirely in harmony with our prediction based upon the principle of dynamogenesis. Therefore, the previous use of that principle in explaining the characteristics of the vowel-pitch triangle is strongly confirmed.

[3] Subject DB, who showed great interest, asked afterward the purpose of the experiment. When told, he said, "You know, I noticed my pitch rising when I squeezed the dynamometer, and I tried to bring it down." His record shows that he succeeded. No squeeze = 115 vibrations per second, squeeze = 112.8. His record was thrown out as not typical.

TABLE V

PITCH OF "SAT" IN DYNAMOGENESIS EXPERIMENT

Subj.	No Dyn.	Dyn.
SR	118.2	121.3
RPW	182.5	201.3
SCW	126.8	133.7
PSS	125.6	144.0
Co	132.4	130.7
HBH	146.5	150.3
Tr	122.1	122.5
JHL	89.1	93.2
BCH	135.6	141.4
IG	111.5	124.6
JJC	134.2	157.4
KvS	103.9	114.3
EEG	146.4	153.4
EHM	116.6	132.9
Mean	128.0	137.2
Difference	9.2	
P.E. diff.	1.32	
Diff./P.E. diff.	6.92	
Probability that true diff. > 0	0.9999	

There remains to consider one other hypothesis which might conceivably account for this new triangle. It would seem possible that some hypothetical influence associated with the characteristic overtones of each vowel might affect the fundamental tone in some manner to produce such differences as we have found. It will be noted from Table I that for the front vowel series, the overtone values closely parallel the fundamental pitch progression we have discovered. But for the back vowel series, it will be seen that the overtone progression is in a direction exactly opposite to the fundamental pitch progression revealed by our data. As an explanation of the vowel-pitch triangle, any such influence of the overtone constants is therefore quite inapplicable.

It is quite possible that in addition to the dynamogenetic diffusion of energy which apparently occurred, another factor may have been operative in causing the rise in the pitch of those vowels which require a high tongue-position. The unconsciously felt rise-in-tongue-position may act as a sym-

bolic pattern and carry over by an unconscious analogy such that the 'rise-in-tongue-position' pattern is felt to require a concomitant 'rise-in-cord-pitch' pattern. However, one test of a hypothesis is that from it the experimental findings shall be unequivocally deducible; and from this standpoint, the concept of analogic symbolic patterns has seemed rather difficult to apply in the present study. If one assumes a 'rise-in-tongue-position' symbolic model, then the actual findings do clearly follow. But one may, with equal *a priori* justification, conceive of a 'rise-in-oral-pitch' symbolic model. By analogy with this model, the cord-pitch pattern should parallel Liddell's spectrum rather than the vowel triangle. The concept of analogic symbolic patterns seems, therefore, in the present instance, to be in the nature of an explanation after the fact. Consequently, while such processes may well have occurred, there is some reason for relying more upon dynamogenesis as an explanatory principle.

Perseveration of Dynamogenesis

It will be recalled that the higher-pitched vowels in Fig. 2 or Fig. 3 represent sounds which are believed to involve progressively greater tongue tension. It seems plausible that the high tension involved in speaking 'seat,' for example, might perseverate for some time, and further *that the tension it induces in the vocal cords might also perseverate.* If this be true, then in the experimental pair 'seat—set' of our series (Table II), 'set' should have a higher pitch than it would, for example, in the pair 'sot—set.' It should in general be true that each vowel is higher in pitch when it follows the highest (most tense) vowel with which it was paired than when it follows the lowest (least tense) vowel with which it was paired. The data relevant to this problem are summarized for the men and women separately in Tables VI and VII. It is clear from these tables that the pitch differences deduced from the principles of dynamogenesis and perseveration are disclosed by the experimental findings; and the size of the critical ratios indicates a fair degree of probability that this perseverational tendency would recur if the experiment should be repeated.

In general, then, a vowel tends to be pronounced at a higher pitch when it immediately follows a vowel which is high on the pitch-triangle than when it follows a vowel which is low on that triangle.

It has been suggested [4] that this tendency may involve the melody patterns of speech as well as the simple perseveration of tension suggested above. If the first word of a pair fixes a given pitch, then the tendency to articulate two such words according to a speech melody more or less constant for the English language would operate to bring the second word up or down to a pitch so related to the first as to preserve that melody.

An interesting observation may be made from the 'Diff.' columns of Tables VI and VII. For the front series, the perseverational tendency is most marked at the base of the triangle ('sot') and decreases regularly as one ascends the triangle toward 'seat.' By analogy with pitch-triangle phenomena, one should expect the perseverational tendency for the back series to be greatest at the base of the triangle, whereas actually the progression is in exactly the opposite direction. The regularity of these progressions and their reproduction in the data for both men and women suggest strongly that the phenomenon is a reality, but no explanation of it has yet been formulated. It may be mentioned that this progression consistently reverses the order of the frequency-constants given in Table I, but the connection between these two types of phenomena is not as yet apparent.

It seemed possible that a further test of the perseverational hypothesis might be found in the data of the dynamogenesis experiment. There only the word "sat" was used; and if the vocal-cord tension induced by squeezing the dynamometer should perseverate more than three or four seconds, then a 'no-squeeze-sat' should be higher in pitch immediately following a 'squeeze-sat' than following nothing or a 'no-squeeze-sat.' Letting 'no-squeeze' be 'a' as before, then the 'a's' which are underscored in the following formula should be higher than the 'a's' which are not underscored: a-b-b-a-b-a-

[4] The writer is indebted to Professor Sapir for this alternative explanation.

TABLE VI

PERSEVERATIONAL TENDENCIES, MEN SUBJECTS

No. of Subjects	Vowel	Pitch when vowel follows		Diff.	$\frac{\text{diff.}}{\text{P. E. diff.}}$
		Lowest	Highest		
8	sot	118.1	125.7	+7.6	3.13
8	sat	122.5	127.4	+4.9	3.38
8	set	124.5	127.0	+2.5	1.39
8	sit	136.0	136.9	+0.9	——
8	seat	141.4	142.0	+0.6	——
7	sought	129.0	126.5	−2.5	2.74
7	put	138.0	135.9	−2.1	0.68
7	suit	136.8	141.4	+4.6	2.42
Mean		130.79	132.85	+2.06	
P.E. diff.				0.789	
Diff./P.E. diff.				2.61	

TABLE VII

PERSEVERATIONAL TENDENCIES, NINE WOMEN SUBJECTS

Vowel	Pitch when vowel follows		Diff.	$\frac{\text{diff.}}{\text{P. E. diff.}}$
	Lowest	Highest		
sot	284.6	289.3	+4.7	1.36
sat	281.5	288.5	+7.0	1.83
set	286.5	290.3	+3.8	1.02
sit	303.5	303.3	−0.2	——
seat	297.6	292.3	−5.3	1.24
sought	286.2	285.7	−0.5	——
put	296.6	302.9	+6.3	3.12
suit	298.1	307.5	+9.4	2.34
Mean	291.83	294.98	+3.15	
P.E. diff.			1.072	
Diff./P.E. diff.			2.94	

a-b-b-a-a-b-a-b-b-a, or b-a-a-b-a-b-b-a-a-b-b-a-b-a-a-b. The data were retabulated to test for this tendency in the eleven men for whom one of the above formulae had been recorded in a solid block with no interruptions. Table VIII summarizes these data; and it will be seen that there is some indication of per severation here, as our hypotheses had suggested. It is to

TABLE VIII

PERSEVERATIONAL TENDENCIES IN DYNAMOGENESIS EXPERIMENT

Subject	Pitch of "a" following	
	"a" or o	"b"
JHL	87.8	85.1
SR	116.2	119.3
RPW	176.3	186.3
BCH	135.3	132.5
SCW	128.2	125.8
PSS	124.5	126.3
Co	125.1	135.9
IG	116.8	110.3
HBH	147.2	146.1
Tr	122.1	122.1
JJC	136.1	138.9
Mean	128.7	129.9
Difference	1.2	
P.E. diff.	0.856	
Diff./P.E. diff.	1.39	

be noted, however, that only five of the eleven subjects show perseveration, though the average of all eleven is slightly in that direction. On the other hand, it is to be noted that in the dynamometer experiment, six to ten seconds elapsed between successive vowels, while within the pairs used in previous parts of this study, only one second or so elapsed. This suggests that if the curve of vocal-cord perseveration of tension excited by irradiation of tension in the hand were plotted, the larger part of that curve might be found in the first six to ten seconds following pressure on the hand-dynamometer.

THE TENDENCY TO ANTICIPATION

It will be noted that the preceding section dealt with the question, How is the pitch of a vowel altered when it *follows* a high- or low-pitched vowel? This section deals with the question, How is the pitch of a vowel altered when it *precedes* a high- or low-pitched vowel? It is well-known that habitual action-patterns tend frequently to be displaced forward in time,—that is, tend to occur before their normal position in the series has been reached. If this principle were operative

in our experiments, there should be evidence in our data that the subjects pronounced a given vowel (the first of any pair) at a higher pitch when the immediately following vowel was to be a high one on the pitch-triangle (Figs. 2 and 3) than when the immediately following vowel was to be a low one on that triangle.

Tables IX and X summarize the pitch of each vowel

TABLE IX

ANTICIPATORY TENDENCIES IN PITCH, MEN SUBJECTS

No. of Subjects	Vowel	Pitch when vowel precedes		Diff.	$\frac{\text{diff.}}{\text{P. E. diff.}}$
		Lowest	Highest		
8	sot	134.8	144.2	+9.4	6.04
8	sat	140.2	142.8	+2.6	0.74
8	set	149.2	153.1	+3.9	1.59
8	sit	155.0	164.4	+9.4	3.40
8	seat	163.1	156.8	−6.3	2.15
7	sought	145.4	144.1	−1.3	0.45
7	put	162.6	159.7	−2.9	1.49
7	suit	166.4	166.9	+0.5	——
Mean		152.09	154.0	+1.91	
P.E. diff.				1.25	
Diff./P.E. diff.				1.53	

TABLE X

ANTICIPATORY TENDENCIES IN PITCH, NINE WOMEN SUBJECTS

Vowel	Pitch when vowel precedes		Diff.	$\frac{\text{diff.}}{\text{P. E. diff.}}$
	Lowest	Highest		
sot	305.3	307.7	+2.4	0.47
sat	314.1	313.3	−0.8	——
set	320.1	317.1	−3.0	0.85
sit	331.1	333.5	+2.4	0.43
seat	344.3	345.1	+0.8	——
sought	319.8	318.8	−1.0	——
put	319.2	330.1	+10.9	1.95
suit	343.1	342.8	−0.3	——
Mean	324.625	326.050	+1.425	
P.E. diff.			0.944	
Diff./P.E. diff.			1.51	

when it preceded respectively the lowest and highest vowels with which it was paired. Inspection of these tables indicates that the subjects did to some extent anticipate a high or low second vowel in a pair by raising or lowering respectively the pitch of the antecedent vowel. Just what the nature of that anticipation may be is a deeper question. From the kymograph records we have only the fact that the fundamental pitch of 'sot,' for example, is higher when 'seat' follows it than when 'sat' follows it. But the anticipation may not have been psychologically or physiologically a primary anticipation of higher pitch. It may have been a 'tongue-anticipation,' such that the tongue was actually higher in pronouncing 'sot' before 'seat' then in pronouncing it before 'sat.' In that case, 'sot' before 'seat' would be approaching the 'sat' region in (1) tongue position, and (2) overtone quality as well as in (3) the rising fundamental pitch which we have found. If this be the nature of the anticipation, it could be demonstrated experimentally by using tongue bulbs or X-rays on the physiological side, or Fourier analysis on the acoustic side,—perhaps even ear-analysis if the change were sufficiently large. We have from our present data only the suggestion that anticipation does occur as indicated by the rise in fundamental pitch of a vowel preceding a higher vowel in the triangle.

Summary and Conclusions

1. The series of front vowels which appear in 'sot,' 'sat,' 'set,' 'sit,' and 'seat' tend, other things equal, to be pronounced at progressively higher laryngeal pitches.

2. The series of back vowels which appear in 'sought,' 'put,' and 'suit' tend also to be pronounced at progressively higher pitches.

3. There is consequently a vowel-pitch triangle corresponding to the traditional 'vowel-triangle' which was originally based on functional anatomical considerations and had no pitch significance whatever.

4. Since the series of front vowels as in 'sot' to 'seat,' and the series of back vowels as in 'sought' to 'suit,' both involve progressively increasing degrees of tension in the tongue

musculature, the most plausible explanation of the vowel-pitch triangle appears to be that it results from dynamogenetic irradiation of the tongue tension to the vocal-cord musculature.

5. Further justification for thus invoking the principle of dynamogenesis is given by the evidence, immediately deducible from that principle and clearly borne out by the experimental findings, that tension in the hand musculature is concomitant with a rise in the pitch of the voice.

6. A vowel tends to be pronounced at a higher pitch when it follows a vowel which is high on its side of the vowel-triangle than when it follows a vowel which is low on the triangle. When the first vowel of the pair is high, there is, according to our hypotheses, greater tongue-tension and, through dynamogenesis, greater cord-tension. It is believed that this tension perseverates long enough to raise the pitch of the second vowel of the pair. Speech-melody factors may also be operative in producing this phenomenon.

7. The perseverational rise in pitch noted in (6) is progressively less for the high front vowels, but progressively greater for the high back vowels.

8. The explanation of (6) as due to perseveration is somewhat strengthened by an indication that a given vowel tends to be pronounced at a higher pitch immediately after a hand-dynamometer has been squeezed than at a somewhat longer time afterward. Here the vocal-cord tension induced by irradiation from the hand-tension apparently perseverates long enough to raise the pitch of the immediately following vowel.

9. It is believed that the cord-tension induced by full-strength pressure on the hand-dynamometer will be so weakened as to produce no appreciable rise in vocal pitch, within six to ten seconds following that pressure.

10. A vowel tends to be pronounced at a higher pitch when it precedes a vowel which is high on its side of the triangle than when it precedes a vowel which is low on the triangle. This is interpreted as an anticipatory reaction.

11. Since any emotional state presumably involves a

noticeably heightened activity and tension of a number of muscle groups, particularly the smooth muscles, it seems plausible that this tension might irradiate to the vocal cord musculature, in the same manner as we have found irradiation to the cord from tongue tension and hand tension. There evolves, therefore, the interesting hypothesis that, apart from any actual speech context which might involve socially stereotyped speech behavior of some opposing character, any marked emotional arousal should be accompanied by a rise in the pitch of the voice. While general observation appears to confirm the phenomenon here deduced, it should be relatively easy to test experimentally by means of the general techniques employed above, together with the application of some strong electric shocks or other painful stimuli either at the moment of speaking the vowel or immediately thereafter.

An article which appeared after the present study had been submitted for publication, offers further evidence for the correctness of the traditional vowel-triangle. See Parmenter, C. E., and Treviño S. N., Vowel positions as shown by x-ray. Quart. J. Speech, *18*, June, 1932, 351–369.

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NEGATIVE INSTANCES IN CONCEPT LEARNING

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Sometimes a concept is learned from a group of stimulus patterns all of which are members of the class in question. Thus, a child learns that the animals in a certain cage at the zoo are 'lions,' the marchers in a parade are 'communists,' the flowers in a vase are 'roses.' Much of the time, however, concept learning proceeds on the basis of stimulus patterns that are not arranged in an orderly fashion in the environment. A small boy learns that some of the marbles in his bag are 'agates' and some are not, some of the automobiles passing in the street are 'coupes' and some are not, some of the heavenly bodies are 'planets' and some are not. Although grouping is involved in concept formation, the objects in the environment do not always form groups that correspond with our classifications of them; the 'lion' may lie down with the 'lamb.' In short, a concept may be learned from either (1) positive instances only or (2) both positive and negative instances.

We have reported a study of the relative merits of these two methods of concept learning when *serial* presentation of the learning material is employed. It there appeared that "negative instances are not necessarily aids to rapid learning. Some subjects did appear to learn from them, it is true, but others seemed to find them a waste of time or even a source of confusion. However, there was no evidence of two or more rigid 'types.' The evidence suggested the possibility that further experimentation might find learners scattered in a form approximating a bell-shaped curve, most individuals learning something from negative instances and a progressively smaller number of individuals approaching the extremes of learning everything or nothing from them." [1]

[1] An objective study of concept formation. *Psychol. Monog.*, 1932, *42*, No. 4, p. 43.

It was the purpose of the experiment here reported to investigate the relative merits of concept learning from (1) positive instances only and (2) both positive and negative instances when *simultaneous* presentation of the learning material is employed.

Six concepts were used in this study. Each concept concerned a certain type of geometrical design. The concepts were as follows:

'Wez': two straight lines of equal length that touch on end together with a dot equidistant from the free ends of the lines.

'Gid': a green triangle and a blue square, the triangle being within the square but not touching it.

'Vec': an equilateral triangle together with a line which is perpendicular to one side and which extends from that side to the vertex of the opposite angle.

'Dax': a circle and two dots, the one dot being inside the circle and the other outside of it.

'Mib': a triangle and a line extending at right angles from its shortest side.

'Zum': three straight red lines, two of which intersect the third, thereby trisecting it.[2]

Designs of these six types were drawn on three-ply gray Duro Bristol cards three inches square in Higgins' orange, brick red, green, blue, and india ink. In order to be true to ordinary learning situations, they were made unlike each other in size, color, position, and width of line. First we shall discuss the designs used in learning, then those used in testing.

Eight 'wez' designs (positive instances), and eight designs that somewhat resembled a 'wez' but that violated at least one condition essential to 'wez' (negative instances), were drawn on cards as mentioned above. Four of the eight negative instances violated only one condition essential to 'wez'; the other four violated at least two conditions essential to 'wez.' The sixteen cards were then shuffled and pasted on a sheet of heavy cardboard fifteen inches square in rows of four in 'chance' order. This process was repeated with 'vec,' 'dax,' 'mib,' and 'zum' designs.

Sixteen 'gid' designs were drawn on cards which were then pasted on a sheet of paper as before. This process was repeated with 'vec,' 'dax,' 'mib,' and 'zum' designs, eight of the designs in each of these four cases being duplicates of the eight positive instances to which reference was made in the preceding paragraph. The arrangement of these duplicate designs on the cardboard base was in each case the same as that of the designs of which they were copies.

Thus, in addition to the group of 'wez' designs and the group of 'gid' designs ('wez' and 'gid' were merely illustrations used for the purpose of familiarizing the subject with the experiment), two groups of sixteen designs each were developed for 'vec,' for 'dax,' for 'mib,' and for 'zum,' the two groups in each case being alike except for the fact that eight negative instances in the one group took the place of eight positive instances in the other. A plus sign was drawn under each positive instance, a minus sign under each negative instance.

A test series was developed for each of the six concepts, a series consisting of sixteen designs, each drawn on a card that had been eyeleted so as to fit a standard office

[2] The use of these concepts was tested, and their relative difficulty was determined, in the study to which reference has been made.

calendar file. About half of these sixteen designs were positive instances; the others were negative instances. These two types of designs were mixed in 'chance' order, and each card was numbered and placed in its position in the calendar file. The negative instances were drawn in such a way that (1) one and only one condition essential to the concept in question was violated in each design, and (2) every condition essential to the concept was violated at least once.[3] A glance at the responses made by the subject while going through the test series was therefore all that was needed to discover what he had failed to learn, if anything.

Thirty undergraduates served as subjects in this experiment. Each subject came for one experimental period. Each of the subjects listed in the column to the left in Table 1 was given the following instructions in typewritten form at the beginning of his experimental period:

"You will see sixteen geometrical designs or drawings. Every design has either a plus sign or a minus sign under it. Each design that has a *plus* sign under it *is* a 'wez,' and each design that has a *minus* sign under it *is not* a 'wez.'

"*Try to find out what a 'wez' is.*

"*When you think that you know what a 'wez' is, raise your hand.*

"The experimenter will record the time that you take, but do not hurry. Take as much time as you need. Yet do not hesitate overlong.

"After you have learned 'wez' you will be shown some more designs and will learn 'gid,' 'vec,' 'dax,' 'mib,' and 'zum.' In the case of 'wez,' 'dax,' and 'zum' some of the designs will be marked with a plus sign and some will be marked with a minus sign, that is to say, some of the designs will be 'wezs,' 'daxs,' and 'zums' and some of them will not. In the case of 'gid,' 'vec,' and 'mib' all of the designs will be marked with a plus sign, that is to say, there will be no designs that are not 'gids,' 'vecs,' and 'mibs.'

"'Wez' and 'gid' are merely illustrations.

"Please reread the above instructions until you are absolutely sure that you understand everything. If you do not see what is meant by any point, ask the experimenter about it."

The subjects listed in the column to the right in Table 1 received the same instructions except that the fifth paragraph was as follows:

"After you have learned 'wez' you will be shown some more designs and will learn 'gid,' 'vec,' 'dax,' 'mib,' and 'zum.' In the case of 'wez,' 'vec,' and 'mib' some of the designs will be marked with a plus sign and some will be marked with a minus sign, that is to say, some of the designs will be 'wezs,' 'vecs,' and 'mibs' and some of them will not. In the case of 'gid,' 'dax,' and 'zum' all of the designs will be marked with a plus sign, that is to say, there will be no designs that are not 'gids,' 'daxs,' and 'zums.'"

[3] It was not deemed necessary or desirable always to adhere to this rule rigorously. In drawing the negative instances for the 'zum' test series, for example, the lines were never made to zig-zag or curve.

When the subject indicated that he understood the instructions, the experimenter handed him the cardboard sheet containing the 'wez' designs and at the same time started a stop watch. When the subject raised his hand (thereby indicating that he thought he had learned the concept) the experimenter stopped the stop watch and recorded the time on a time card. The time spent by the subject in examining the designs was taken as the measure of the rapidity with which he learned the concept.

The experimenter then handed the subject a sheet of paper and a pencil, and asked that he draw three 'wezs.' If the drawings were not accurate representations of 'wez' (within the limits of accuracy in free-hand drawing) the subject was told what a 'wez' is. If the drawings were accurate representations of 'wez' the experimenter handed the subject a sheet of paper on which columns of numbers from one to sixteen had been mimeographed and the calendar file containing the 'wez' test cards. The experimenter said, "Here is a series of sixteen drawings. Some of them are 'wezs' and some are not. If the drawing is a 'wez' write 'Yes' after its number. If it is not, write 'No.'" If the subject made no mistakes in going through the test series the experimenter knew that the subject knew what a 'wez' is. If the subject made mistakes, the experimenter told him what a 'wez' is. If the subject did not learn what a 'wez' is within three minutes, the experimenter told him.

After the subject had learned 'wez' he learned 'gid' (which, it will be recalled, was also merely an illustration), 'vec,' 'dax,' 'mib,' and 'zum' in the same manner, except that in the case of 'vec,' 'dax,' 'mib,' and 'zum' (1) the time limit was five minutes and (2) the subject was not told what the concept in question was unless the time limit had expired. If the subject failed on the drawing test for 'vec,' 'dax,' 'mib,' or 'zum,' or, having passed the drawing test, made a mistake in going through the test cards, the experimenter handed the subject the cardboard sheet containing the learning cards for the concept in question and said, "You know something about '——' but you do not know everything; we shall try it again."

Half of the subjects learned 'vec' and 'mib' from positive instances only and 'dax' and 'zum' from both positive and negative instances. The other subjects learned 'vec' and 'mib' from both positive and negative instances and 'dax' and 'zum' from positive instances only. Subjects were placed in these groups in 'chance' order, alternate subjects being members of a group.

The two methods of learning were alternated for each subject, and the order in which the subjects learned the four concepts was varied. Thus, subjects A and B learned the concepts in the order indicated in Table 1, reading from left to right; subjects C and D learned 'dax' first, then 'mib,' 'zum,' and 'vec'; and so on. The experimenter said, "Now we shall learn '——' by the plus (or the plus and minus) method" before the subject examined the designs for any given concept.

It is clear from Table 1 that there is here no statistically

TABLE 1

SHOWING THE NUMBER OF SECONDS SPENT IN EXAMINING THE DESIGNS (I) AND THE NUMBER OF TIMES THE SUBJECT INDICATED THAT HE THOUGHT HE HAD LEARNED THE CONCEPT (II)

Subject	Vec (P)		Dax (P, N)		Mib (P)		Zum (P, N)		Subject	Vec (P, N)		Dax (P)		Mib (P, N)		Zum (P)	
	I	II	I	II	I	II	I	II		I	II	I	II	I	II	I	II
A.....	39	1	37	1	41	1	x	1	B...	38	1	33	1	83	1	x	2
C.....	63	1	81	1	x	3	x	2	D...	37	1	23	1	48	1	x	4
E.....	80	1	66	1	x	2	x	0	F....	25	1	37	1	30	1	x	3
G.....	40	1	13	1	44	1	x	0	H...	29	1	89	1	x	1	x	2
I.....	14	1	19	1	16	1	60	1	J....	36	1	45	1	25	1	x	3
K.....	69	2	42	1	x	1	x	2	L....	70	1	61	1	x	0	x	2
M.....	37	1	83	1	x	2	x	0	N...	64	1	62	1	x	1	x	2
O.....	21	1	25	1	66	1	x	0	P....	52	1	54	1	86	1	x	2
Q.....	149	2	69	1	x	2	x	1	R...	31	1	49	1	67	1	x	1
S.....	12	1	23	1	130	3	x	1	T...	20	1	22	1	35	1	252	2
U.....	35	1	40	1	90	2	x	2	V...	20	1	109	1	239	2	209	3
W.....	29	1	40	1	244	2	x	3	X...	34	1	36	1	268	2	x	2
Y.....	22	1	25	1	x	3	x	1	Z....	45	1	33	1	x	1	x	1
AA.....	44	1	30	1	32	1	x	0	BB..	28	1	12	1	68	2	x	2
CC.....	35	1	43	1	52	1	x	1	DD..	26	1	28	1	26	1	x	1
Mid-score..	37		40		130		x			34		37		83		x	
Total...		17		15		26		15			15		15		17		32

(P): Series consisted of positive instances only.

(P, N): Series consisted of both positive and negative instances.

x: Concept was not learned within five minutes.

significant difference in the time required for learning concepts from both positive and negative instances as compared with learning from positive instances only. (This result was also obtained in the study previously reported wherein serial presentation of the learning material was employed.) This of course does not mean that every learner does equally well under the two sets of learning conditions; it merely means that there is here no evidence of a distinct tendency among learners to learn concepts with greater rapidity by the one method than by the other.

Most of the subjects, however, did *prefer* to learn from both positive and negative instances. When asked at the conclusion of the experiment which method they preferred, twenty-three of the thirty subjects expressed a preference for learning from both positive and negative instances, six expressed a preference for learning from positive instances only, and one stated that he had no preference. Their remarks varied from "plus and minus is much easier" (subject F) to "I didn't pay any attention to the minus ones" (subject K). No subject stated that he found the negative instances confusing. (The study to which reference has been made showed that in the case of serial presentation of the learning material negative instances are confusing to some subjects; perhaps this confusion is less likely to exist in the case of simultaneous presentation because of the increased opportunity for comparison and contrast.)

Table 1 also indicates that there is a tendency for negative instances to discourage 'snap judgments.' When the subjects were learning from both positive and negative instances they tended to come to an initial wrong conclusion less readily, and to subsequent wrong conclusions less frequently, than when they were learning from positive instances only. Thus, *although negative instances may not make for rapidity in learning they tend to make for accuracy, especially in the case of difficult concepts. It appears that in so far as negative instances assist concept learning they do so largely because of the way in which they prevent the learner from coming to one or more erroneous conclusions while he is still in the midst of the learning process.*

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THE EFFECT OF ALCOHOL ON THE RATE OF PAROTID SECRETION

BY A. L. WINSOR AND E. I. STRONGIN *

The action of alcoholic beverages on the human organism has been the object of numerous experiments by scientists in various fields. A summary of the diversified literature on this subject reveals two types of investigation—those dealing with the general, and those with the more specific aspects of alcohol. The general studies have concerned themselves with such problems as the effect of alcohol on growth, on stability, on longevity, on ability to resist disease, and on ability to persist or endure in work requiring effort. The more specific studies have endeavored to determine the effect of alcohol on such functions as motor coördination, on body temperature, on pulse rate, on ability to attend, on ability to recall, or on ability to solve problems. In addition to these studies, other investigations have been conducted to determine the extent to which the effects of alcohol were influenced by such factors as sex, age, the previous habits of the individual, the methods of ingestion of the beverage, and the condition of the stomach. Incidental observations during these investigations have revealed certain characteristics of the alcoholic effect such as the elapsed time between the ingestion of alcohol and the onset of the effect or the duration and time of maximum intensity of this effect.

All of these studies, many of which are contradictory in their conclusions, are clear in their recognition of one phenomenon in connection with the use of alcohol which contributes to the unreliability of their data. This universal characteristic is the capacity of the human organism to develop a tolerance for alcohol. Habituated and non-habituated individuals exhibit not merely quantitative but

* The investigation upon which this article is based was supported by a grant from the Heckscher Foundation for the Advancement of Research established by August Heckscher at Cornell University.

qualitative differences in reaction subsequent to the ingestion of a given dosage of alcohol. While experimenters have been aware of this capacity of the organism to partially adapt itself to alcohol, they have been at a loss to know just how much weight this factor should be given. The extent to which tolerance for a drug may be developed; the time or dosage necessary to effect this tolerance; individual differences in capacity to develop tolerance; the effect on the rate of development of such factors as age, sex, weight, distribution of dosage, and frequency of distribution of dosage; are phases of the problem hitherto unexplored. Until these problems are solved and taken into account, much of the research in this field will continue to be unreliable and contradictory. It has seemed to us, therefore, that satisfactory research with alcohol is dependent upon a better understanding of this basic adaptive process. Hitherto little research has been done in this field and those studies that are reported deal primarily with chemical changes. The reason for this neglect may be due to the fact that adequate observation and control for such an experiment demands a prolonged study of progressive changes that have been generally assumed to be continued for a protracted period.

After observing the development of tolerance for caffeine beverages [1] and cigarettes we were convinced that similar studies might be conducted with alcohol. We had found after some exploring that the development of tolerance could be observed either in progressive changes in motor reactions or secretory reactions. In the case of alcohol, the secretions changes seemed to offer certain advantages. The rate of secretion is not only more definitely measurable, but the secretions of digestion are more immediately concerned with drugs both directly and indirectly than are the muscles.

Our first problem, of course, was to establish definitely the nature and extent of the influence of alcohol on the rate of secretions. While some observations of the effect of alcohol on the rate of gastric secretion had been made, little information was available on the effect of this drug on salivary secretion. Judging from the observations of those who drink

[1] In press.

alcoholic beverages intermittently, one would expect that the effect of alcohol is inhibitory. The recurrence of a thirst sensation in spite of the ingestion of large quantities of liquid and the "dark brown" taste on the "morning after" indicated lack of normal functioning of these glands. However, these observations needed verification under controlled conditions. In attempting to establish these facts for our tolerance experiments, data were obtained relative to the general effect of alcohol in any form on secretory activity which seem to be of sufficient importance to justify consideration in a separate report. These data are presented in this paper as a preliminary study to the development of tolerance for alcohol.

Since the technique employed for the collection and measurement of the secretion was the same as that already reported for the studies of the effect of tobacco [2] and caffeine on secretion, detailed consideration of this phase of our report may be dispensed with. It is sufficient to indicate that a small silver disc similar to the one developed by Lashley [3] is fastened by suction over the mouth of Stenson's duct. From the inner chamber of this disc a small airtight drainage tube passes through the corner of the mouth carrying the secretion to a device capable of measuring it in hundredths of a cc. The collecting device itself does not affect the flow of secretion or seriously interfere with the mouth movements of the individual. The sensitiveness, harmlessness, and simplicity of this apparatus as well as the accuracy with which variation in reactions may be measured makes it particularly useful for experiments of this type.

Male and female adults were employed as subjects who had served in other experiments and were accustomed to the apparatus. The experiments were conducted in a quiet laboratory free from interruption or disturbance of any sort. The subject was seated in front of a small table and instructed to assume a comfortable posture. After the disc was properly adjusted he was instructed to avoid excitatory movements

[2] A. L. Winsor, The Effect of Cigarette Smoking on Secretion, *Jour. of Geol. Psychol.*, 1932, 6, 190.

[3] K. S. Lashley, Reflex Secretion of the Human Parotid Gland, J. EXPER. PSYCHOL., 1, 1916, 461–493.

of the tongue or throat, except as directed, and to keep his mouth closed. In order that accumulated secretions from other glands of the mouth might not interfere with the throat movements, the subject was permitted to swallow at the end of every five-minute period.

Before the alcohol was ingested the normal rate of secretion was measured for fifteen minutes. In years of experimenting with the rate of secretions for these glands, we had observed among other things that physical activity of any sort causes an increase in the rate of normal secretion. Hence it was necessary for the subjects to remain quiet for ten or fifteen minutes before the flow subsided to its normal rate. This rate served as a control flow for the tests which followed. The dosage of alcohol was then consumed within a one-minute period and the subject resumed his posture at the table and the rate of secretion was recorded at the end of every five-minute period for the next two hours.

Because of the exploratory nature of this preliminary experiment the nature of the dosage was varied. Grape wines, whiskey, and ethyl alcohol were all used in various concentrations and in different quantities. Some tests were made immediately after a full meal while others were preceded by an eight-hour fast. None of the subjects were habitual users of alcohol in any form. Interesting differences as between alcoholic beverages were observed, but further experimentation with larger groups is necessary to validate these findings before they are published. We are interested here in the general effect of alcoholic beverages.

Figure 1 is a graphic representation of the variation in secretion when a male adult was given 65 cc of bonded whiskey forty-five minutes after he had eaten dinner. The ordinate represents the secretion in terms of ccs and the abscissa represents successive five-minute periods. Beginning on the left the first three columns, showing a rate of approximately 14 cc per five-minute period, represent the control flow. The next long broken column represents the period in which the beverage was ingested. To show the actual relative height of this column it would be necessary to increase the column

five times its present height. It represents the direct reaction from stimulation of the end organs in the mouth and throat. Since we were not so much concerned with the results of this contact with alcohol on the mucosa, we may disregard the exact amount of secretion registered during this period.

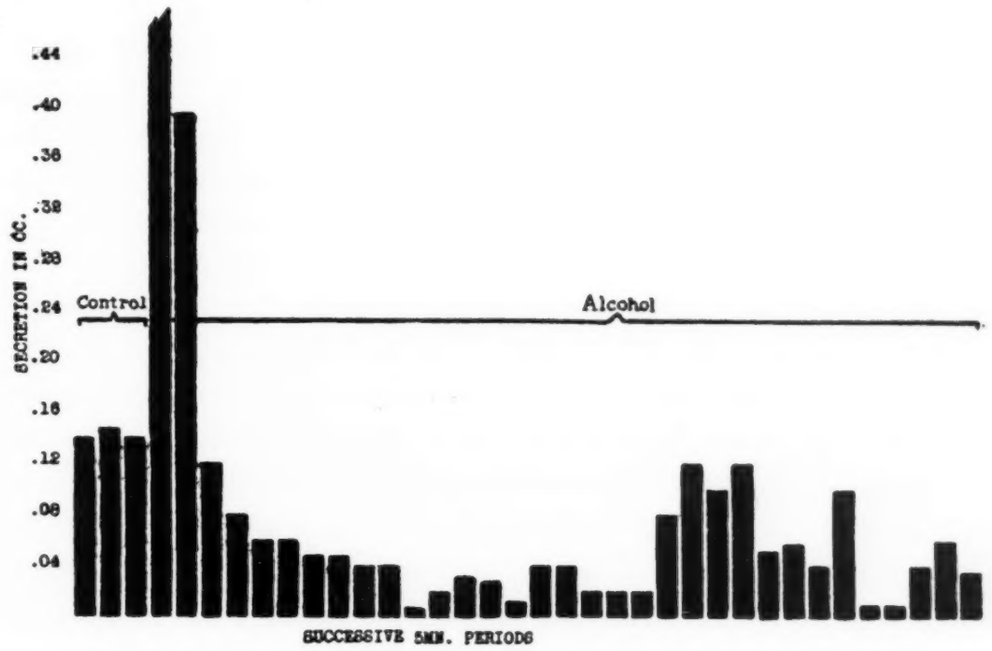

FIG. 1. Effect of an alcoholic beverage on the rate of parotid secretion.

According to Pavlov [4] the biological purpose of this large quantity of fluid is to dilute the noxious substance or form chemical compounds with it and thus protect the mucous membrane. The next column shows the effect of this direct stimulation continuing into the following period. Thereafter the graph shows a gradual decrease in secretion for successive five-minute periods for forty-five minutes. The subsequent forty-five minutes show a definitely inhibited flow of secretion and this is followed by a period of forty minutes when the secretion again approaches normal. Toward the end of the graph there appears to be a recurrence of inhibition.

As far as secretion is concerned, it would appear from these data that alcohol may be classed as a stimulant only when in actual contact with the end organs located in the walls of the digestive tract. Here is a new line of evidence to be added to

[4] I. P. Pavlov, The Work of the Digestive Glands. Lippincott, 1910, p. 238.

the collection already made which contradicts the popular idea that credits alcohol with 'stimulant' properties. Once the alcohol passes into the blood, its action appears to be definitely inhibitory. The intensity and duration of this inhibitory reaction varies with the subject, the condition of the stomach, the rate of ingestion, and the amount of alcohol consumed. The period of greatest inhibition comes usually between 45 and 90 minutes after ingestion of the beverage. In this connection it is interesting to recall the study by Miles [5] of the concentration of alcohol in the blood, plasma, and urine. His results showed that the maximum concentration in both the blood and urine occurred from 60 to 75 minutes after ingestion of the alcohol. These data also support Carlson's [6] observations of the inhibition of hunger contractions. When his subject drank 50 to 100 cc of 10 percent alcohol, the hunger contractions were inhibited from 1 to 2 hours after ingestion.

Since it is alcohol in the blood which intoxicates, it would be expected that any evidences of intoxication would occur during the periods of most intense inhibition. The only times our subjects recorded definite feelings of intoxication were during the periods of marked inhibition. Before the experiment began, a pencil and writing pad was placed on the table and the subject was requested to note any marked changes in feeling. During the periods of greatest inhibition he invariably noted evidences of intoxication. When he reported that he felt normal, the rate of secretion approximated the normal flow.

As a result of Carlson's prolonged study of Mr. V., whom it will be recalled had a permanent gastric fistula through which the functioning of the stomach walls could be observed, a question was raised for which our data may afford a solution. This question pertains to the apparent contradiction of his findings with the commonly observed sensation of hunger subsequent to the drinking of alcoholic beverages. Making tests with wines, beer, brandy, and pure alcohol, Carlson had

[5] W. R. Miles, Alcohol and Human Efficiency, Carnegie Institution Publication, No. 333, 1924, page 215.

[6] A. J. Carlson, The Control of Hunger in Health and Disease, University of Chicago Press, 1916, p. 179.

observed that the ingestion of alcohol in any of these beverages was invariably followed by inhibition of hunger contractions. How were such results to be harmonized with the notion of laymen and some scientists that alcohol sharpens the sensations of hunger? We are all aware of the fact that a cocktail of assured alcoholic content is frequently given before meals for the avowed purpose of stimulating the appetite. Does it also stimulate hunger? Pavlov has recorded an instance from his own experience where a drink of wine seemed to initiate the sensation of hunger the very moment the wine reached the stomach. Though not conclusive, such evidence cannot be thrust aside without consideration. In summing up his observations, Carlson makes the following statement: "Alcoholic beverages can therefore not initiate or increase hunger, since hunger is caused by the stomach contractions and these are inhibited by the alcohol." P. 180.

It should be recalled here that Mr. V., the subject in this study, drank wine and beer occasionally and the conclusions above are made from observations of his stomach. One of our subjects repeatedly wrote shortly after the ingestion of wine that he felt definite hunger sensations. Soon after he had recorded these observations he invariably reported nausea, and on one occasion vomited, expelling all of the wine 59 minutes after drinking it. On these occasions there was no inhibition of secretion, but rather a marked increase in the flow during the entire hour. To us these data suggest a solution of the problem raised by Carlson.

There is ample evidence to show that secretions of the digestive system and hunger contractions function coördinately under the sympathetic nervous system. When the secretions are inhibited, hunger contractions are also inhibited. Furthermore, it is well established that secretions as well as stomach muscles play a dual role. As one function they facilitate digestion. As another function they facilitate the expulsion of food through regurgitation. Alcohol seems to inhibit the digestive process but does not inhibit the vomiting process. When alcohol causes nausea which usually precedes vomiting, the individual may experience hunger because of the contractions of the walls of the stomach.

In the case of Mr. V. it seems probable that the alcoholic beverages did not cause nausea. Consequently, the contractions of the walls of the stomach were inhibited and the secretions were inhibited. No hunger would be felt.

In the case of our subject, the alcoholic beverage caused nausea because he was not used to alcohol. When the beverage caused nausea the secretions were not inhibited and it seems logical that the contractions of the stomach walls would not be inhibited, but that the action of the stomach walls as well as the rate of secretion would be augmented. Consequently, the subject felt and reported hunger.

Much of the confusion that exists on this subject is probably due to the fact that many people do not differentiate between appetite and hunger. They assume that the difference, if any exists, is one of degree rather than one of quality. They would insist that a strong appetite is called hunger, and that a mild hunger is called appetite. Students of this problem are agreed, however, that these two experiences are fundamentally different. Physiological and psychological experimenters have definitely localized hunger as a function of the stomach. They have produced both direct and indirect experimental evidence to show that the walls of an empty or nearly empty stomach contract when one feels hungry.

Appetite, on the other hand, has been definitely shown to be associated with the flowing of digestive juices caused by the memory of previous tastes or odors of food. When one feels an appetite, the secretions, noticeably those of the mouth, show a marked increase in rate of secretion. Hence, appetite as well as hunger is capable of exact measurement and while the two functions are interrelated, they should be distinguished. Our experiments suggest that alcoholic beverages taken in moderate dosage may either stimulate or inhibit appetite, depending upon the degree of satisfaction that accompanied its ingestion on previous occasions. They might also either stimulate or inhibit hunger contractions, depending upon the extent to which nausea develops.

Of course, gross individual differences in capacity to

tolerate or withstand the effect of alcohol exists. Habituated users show less inhibition of secretion than non-habituated. As indicated at the beginning of this report, it is the development of this tolerance that we are attempting to analyse. In recent studies with caffeine and nicotine we found that when a small dosage of either drug was administered each day to an unhabituated subject, the effect of the drug was gradually reduced. We expect to perform a similar series of tests with an alcoholic beverage as soon as practicable.

(Manuscript received April 25, 1933)

AN EMPIRICAL COMPARISON OF THE VARIOUS TECHNIQUES USED IN THE STUDY OF THE LOCALIZATION OF SOUND [1]

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Sound localization has been the subject of much research in the psychological laboratories for the last half century. Inasmuch as different experimenters have used different techniques, a knowledge of the reliability and validity of these techniques is essential to a complete interpretation of their results. The following study is submitted as a step in that direction.

Problem.—Our problem was the determination of the relative merit of the following techniques which have been used in the past in the study of the localization of sound:

1. The *pointing technique* in which *O* indicates the apparent location of the sound by pointing with the finger or with a pointer.

2. The *diagram technique* in which *O* designates the apparent location of the sound in terms of the angular displacement from the median plane, or in terms of a diagram previously studied.[2]

3. The *visible perimeter technique* suggested by Ruckmick [3] and used successfully by Mathiesen,[4] in which *O* designates the apparent location of the sound in terms of a visible perim-

[1] In a paper read before the Iowa Academy of Science (Sound localization: a comparison of methods, *Proc. Ia. Acad. Sci.*, **32**, 1925, 399–400), Dr. Christian A. Ruckmick suggested a careful study of the basic techniques involved in research in sound localization. This investigation, done under his personal direction, is presented as a step in the research program there advocated.

[2] Trimble, O. C. Some temporal aspects of sound localization, *Psychol. Monog.*, **38**, 1928, No. 4, 172–225.

[3] Ruckmick, C. A. A new technique for recording sound localization, *Amer. J. Psychol.*, **42**, 1930, 638.

[4] Mathiesen, A. Apparent movement in auditory perception, *Psychol. Monog.*, **41**, 1931, No. 4, 74–131.

eter. In the first and second techniques, *O* is blindfolded, but in the third technique, the experiment is done in the dark, the perimeter being made visible by figures outlined in radium paint.

Apparatus.—Observations were made in a sound-proof room approximately twelve feet cube. *O* sat in a comfortable armchair, the height of which could be adjusted so that the ears of *O* were always in the horizontal plane of the stimulating sounds. A headrest, made of soft strap iron padded with cotton and bent to the approximate contour of *O's* head, prevented the head from being moved during the experiment. The stimulating sounds were produced in a telephone receiver unit suspended from the end of a beam which could be rotated horizontally above *O's* head. As the receiver was rotated to the desired position, it could be locked in place by means of a slot device, thus assuring accurate placement of each stimulus. All of the mechanism was concealed from *O* by a black canopy which formed a concave hemisphere in front of him. The sounds were presented at fifteen different locations in the horizontal plane, *viz.*, directly in front of *O*, and 10, 20, 30, 45, 60, 75, and 90 degrees to both the right and left of the median plane.

The stimuli were of two kinds, namely, a noise and a pure tone. The noise was produced by a six volt storage battery connected with the receiver through an automatic circuit-breaker and step-up transformer (ratio, 1 : 10). This gave a single, loud, sharp "click." The tone was generated by a standard radio oscillating circuit, and was practically a pure tone as revealed by an oscillographic analysis. The pitch used was 240 ~.

A seconds pendulum held at one extreme of its natural swing by an electro-magnet was connected between the tone generator and receiver unit. *E* released the pendulum by interrupting the supply of current to the magnet. During the swing, a wire contact dipped into a pool of mercury, completing a circuit through the receiver and making the duration of the tone exactly one second. When the pendulum returned to its original position, it was caught by the electro-magnet and held in place until again released by *E*.

Need of Refined Techniques.—As an introductory study, *Os* were asked to locate sounds by (1) pointing to them and at the same time (2) describing their apparent locations in terms of a diagram previously seen. The average discrepancy between the positions indicated by the two methods was 6.67 degrees. This figure is based on the results of five *Os*, each of whom gave 75 responses. Near the median plane, the discrepancy was almost zero, but near the aural axis, it increased to fifteen degrees. There was no consistent tendency favoring either method. This study is mentioned here to emphasize the importance of a careful investigation of the basic techniques used in this field of research.

Comparison of the Three Techniques.—For this investigation, 21 *Os* were used, each of whom gave 75 localizations for each of the three techniques making 4,725 trials upon which we based our comparisons. In order to cancel the effect of one technique upon another, the order in which the three techniques were used was varied. The first 12 *Os* completed the experiment with one technique before proceeding to the next. Each of the six possible orders [5] was used for two *Os*. For example, *Os* 1 and 2 did the experiment first by the *pointing technique*, then by the *diagram* and finally by the *visible perimeter technique*. *Os* 3 and 4 started with the *diagram technique*, then used the *visible perimeter technique* and ended with the *pointing technique*. The remaining nine *Os*, instead of completing the experiment by one technique before proceeding with another technique as was done by the first twelve, were allowed to give about 25 localizations with one technique and then turn to one of the other techniques for about 25 trials, and so on until each *O* had given 75 responses for each of the three techniques. The maximum number of localizations made at one sitting was 75. Thus in the averages, the effect of one method upon another, effect of fatigue, practice, primacy, *etc.*, should be cancelled out. These data are summarized in the following table.

The averages in the above table show that the *visible*

[5] Considering the *pointing technique* as technique 1, the *diagram technique* as technique 2, and the *visible perimeter technique* as technique 3, six different orders are possible, viz. 1—2—3, 1—3—2, 2—3—1, 2—1—3, 3—1—2, and 3—2—1.

TABLE I

AVERAGE ERROR IN DEGREES MADE IN THE LOCALIZATION OF SOUNDS

Position of Source of Sound	Pointing Technique	Diagram Technique	Visible Perimeter Technique
90 degrees right	9.6	10.9	3.9
75 " "	5.5	11.9	11.5
60 " "	6.7	14.0	11.4
45 " "	9.3	9.5	7.1
30 " "	8.6	9.0	6.6
20 " "	10.0	11.0	5.6
10 " "	10.0	8.1	6.7
front	6.1	4.4	3.6
10 degrees left	7.8	6.5	3.8
20 " "	8.7	9.5	6.8
30 " "	9.0	9.4	7.3
45 " "	9.3	11.1	7.8
60 " "	8.4	13.6	8.3
75 " "	5.8	12.8	7.5
90 " "	7.8	10.5	4.4
Average	8.2	10.2	6.8

perimeter technique gave the more accurate localizations, being 33 percent more accurate than the *diagram technique*, and 17 percent more accurate than the *pointing technique*. The distribution of errors on either side of the median plane is fairly symmetrical for each technique. All techniques show the same general tendency to be most accurate near the median plane and to decrease in accuracy as the sound is displaced from the median plane. A slight rise in accuracy is noticed as the sound reaches the aural axis. Most *Os* stated a preference for the *visible perimeter technique*, saying that the blindfold tended to distract and disorientate them.

Effect of Motivation.—Immediately after going through a series of localization, 12 *Os* were told, "Now I want to determine how quickly you are able to learn. I will give you a few more sounds and compare this record with your first record. Please do your very, very best." Fifteen stimuli were then given and compared with the fifteen just before the incentive was given. In all cases an improvement took place, the amount being shown in the following table.

TABLE II

IMPROVEMENT IN LOCALIZATIONS AFTER AN INCENTIVE WAS GIVEN

Techniques Used	Improvement In Degrees	Improvement In Percent
Pointing	1.5	17
Diagram	1.3	12
Visible perimeter	1.2	16

Effect of Practice.—11 persons observed for this experiment. Each was given a preliminary test of 15 stimuli, then a practice series of 75 trials, and then a final test of 15 stimuli. The improvement of the final test over the preliminary test is given as the effect of the practice periods.

TABLE III

EFFECT OF PRACTICE IN THE LOCALIZATION OF SOUND

Techniques Used	Improvement In Degrees	In Percent
Pointing	3.3	33
Diagram	3.2	27
Visible perimeter	3.9	44

Comparison of Tones and Noises.—The usual series of 75 localizations for each of three techniques was used to determine the relative difficulty of the tone and noise. After nine persons had observed, the data showed no significant differences in relative difficulty for the three techniques, so this part of the investigation was abandoned. For each technique the tone appeared to be about 60 per cent more difficult to locate than the noise.

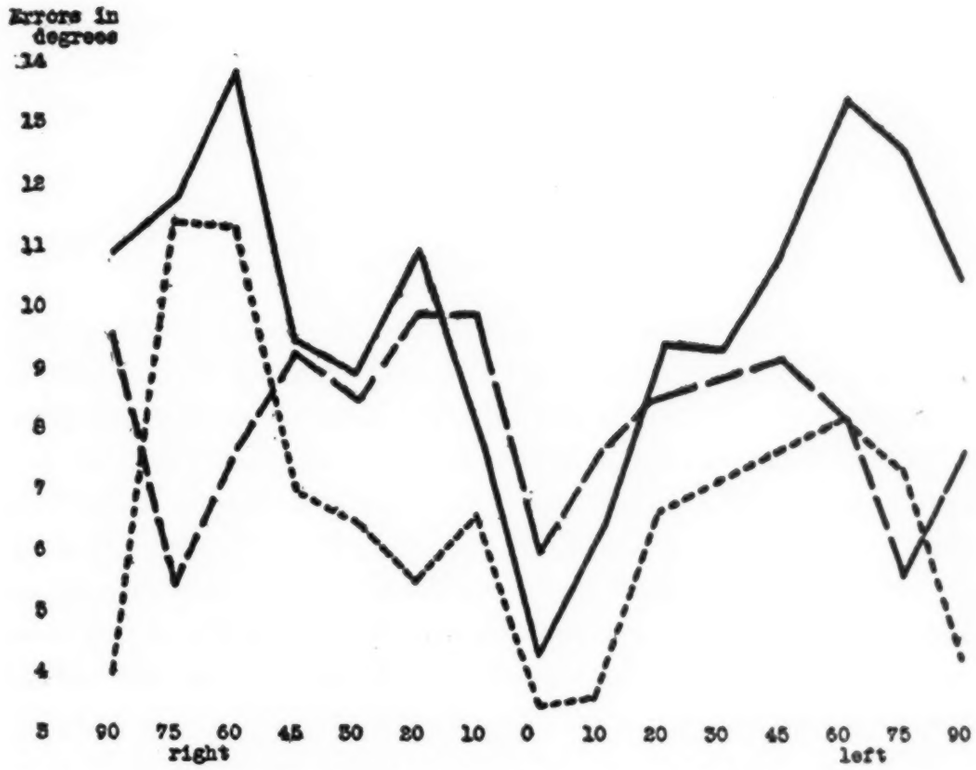
Summary and Conclusions.—The results of this study may be summarized as follows:

1. Techniques which have been frequently used in the past in the investigation of sound localization are badly in need of refinement and improvement as was shown by the fact that the localizations of a single sound by the *pointing* and *diagram techniques* employed simultaneously differed one from the other on an average by 6.67 degrees, with no consistent tendencies favoring either technique.

2. The *visible perimeter technique* proved to be 34 percent more accurate than the *diagram technique*, and 17 percent more accurate than the *pointing technique*.

3. In all positions except those 60 and 75 degrees on either side of the median plane, the *visible perimeter technique* excelled both other techniques. In these positions, the *pointing technique* gave as good and slightly better results.

4. The *pointing technique* was more accurate than the *diagram technique*, except in the front position and ten degrees to either side of the median plane.



Graphs showing the distribution of errors for each of three methods of sound localization

——— Diagram Technique
---------- Visible Perimeter Technique
- - - - - - - Pointing Technique

5. Most *Os* expressed a preference for the *visible perimeter technique*.

6. A tonal stimulus was consistently about 60 percent more difficult to locate than a noise stimulus.

7. Motivation had the greatest effect on the *pointing technique*, causing an improvement of 17 percent.

8. The greatest amount of improvement in learning took place with the *visible perimeter technique* (44 percent) and the least amount (27 percent) with the *diagram technique*.

In short, the *visible perimeter technique* gives the most accurate results, permits the greatest degree of learning, gives the most consistent results, and is preferred by practically all *Os*.

(Manuscript received April 25, 1932)

A HIGH-SPEED CHRONOGRAPH *

BY WALTER VARNUM

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There is a real need for a more perfect means of accurately and efficiently measuring short time intervals. The standard chronoscopes such as the Hipp or the Dunlap are exceedingly complicated in use, require constant adjustment and do not provide permanent records of the measurements. While the synchronous motor chronoscopes can be greatly simplified by using the alternating currents which are now carefully regulated over city power lines, such measures usually can not be made of less than 1/120 second since few circuits use more than a 60-cycle frequency.

In recent years the use of photographic film has come into vogue in the form of pendulum and other types of photochronographs. This type of instrument is essential to certain kinds of work, but it is always laborious and slow and costly to a point of being prohibitive for most quantitative work. Because of the great cost of the film the speed of the driving mechanism has been reduced to such an extent that the lines representing the reaction times are sometimes too small to be read accurately. The instrument described below is inexpensive in its first cost, permits measures to be taken as rapidly as desired, costs almost nothing to use and has demonstrated that it not only records with great accuracy but that the records are of sufficient length to be read without error.

The commercial production of a sensitized recording paper (**5**) has made possible the application of the familiar principle of the electric signal marker in the construction of an instrument for graphically recording differences in time intervals of one sigma or less.

Garrett has pointed out (**4**) that the electric marker, recording together with an electric tuning-fork on a kymo-

* From the laboratory of the University of Southern California.

graph, is accurate to the point of its being 'used to check other mechanical measuring devices.' The chronograph, which was, as Boring (**1**) tells us, 'simply the forerunner of the modern kymograph,' was one of the instruments used in astronomical observatories in dealing with the 'personal equation.' The work of Rosenheim (**6**) on the knee-jerk offers an example of the careful use of this principle in early studies. His study gives a good description of this technique by means of which he was able to measure differences in latencies as short as .025 second. The present chronograph overcomes the objection to the older application that it 'is slow and cumbersome,' (**4**) and in addition possesses numerous advantages of its own.

Construction of the Apparatus.—The chronograph as now built consists of a 1/4 horsepower constant speed motor, *A* in Fig. 1, connected directly to a 48 to 1 speed-reducer, *B*, and thence by means of a belt to the recording drum *C*. The recording drum is a carefully turned, hollow, wooden cylinder 965 mm. in circumference by 150 mm. in width. By means of various pulley ratios it can be turned at speeds of from 25 to 150 r.p.m. About this drum is fastened the recording paper which is lightly coated with a waxlike preparation on one side.[1] The slightest scratch on this surface results in the production of a clear and permanent mark. Above the drum a recording stylus is supported on a spiral feed, *D* of Fig. 1, so that it can move back and forth across the drum. As many styli as simultaneous measures are desired may be used and they can be moved across the drum mechanically by means of a pulley from the drum-shaft, in the case of serial reactions, or turned up a little after each measurement in recording single reactions. The writer has found that excellent styli can be constructed by removing the bell from an electric door bell, filing off the hammer of the striker and bringing its arm to a point. The wiring can then be changed so that it functions as a simple electro-magnet. An inexpensive and accurate marker is thus available which can be used singly in make and break circuits, or in pairs in double break circuits.

[1] The sensitized paper may be had from the Stylograph Corp., Rochester, N. Y.

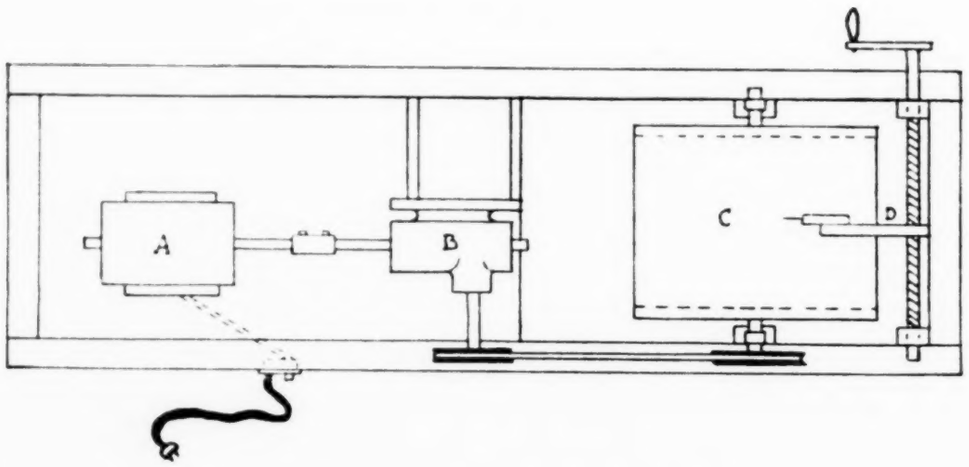

FIG. 1. Top view of chronograph. *A*, Electric motor; *B*, speed reducer; *C*, recording drum; *D*, electric marker.

The apparatus is connected in reaction circuits as any chronoscope might be. Figure 2 is a diagram of the circuit used in determining the latency of the knee-jerk. It is to be noted that the marker is not in constant contact with the drum but is poised above it so that the point of the stylus is 1 mm. from the drum's surface. In this way the stylus is pulled down against the coated paper on the drum by the action of the

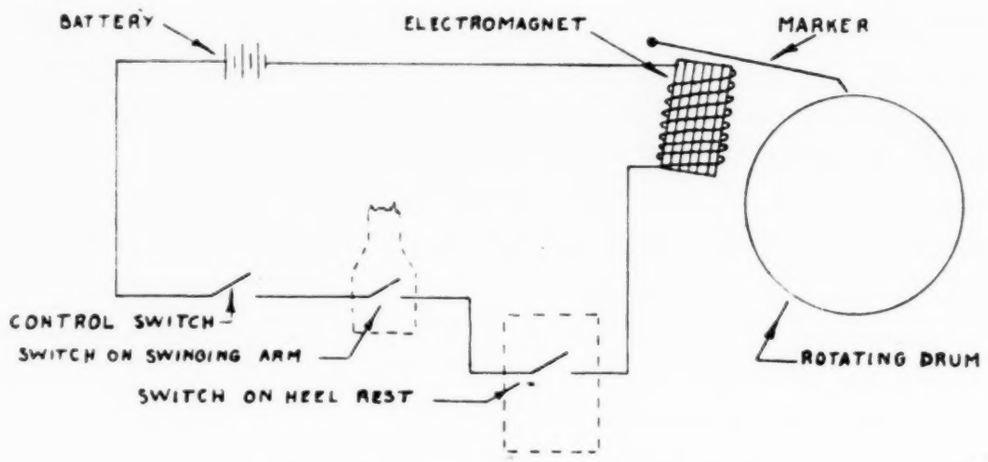

FIG. 2. Diagram of electric circuit used.

electromagnet when the circuit is closed. It thus cuts a line through the wax during the interval between the stimulus and the response. Upon the breaking of the circuit by the release of the response key, the marker is de-magnetized and the stylus springs away from the surface, leaving a short, straight line.

The time intervals represented by these lines (Fig. 3) made on the recording paper may be read in either of 2 ways. A time line, as shown in Fig. 3, is run along the edge of the paper by holding the stylus at the end of an electric tuning fork lightly against the paper during one revolution of the drum. A far more efficient method, if the driving motor offers suffi-

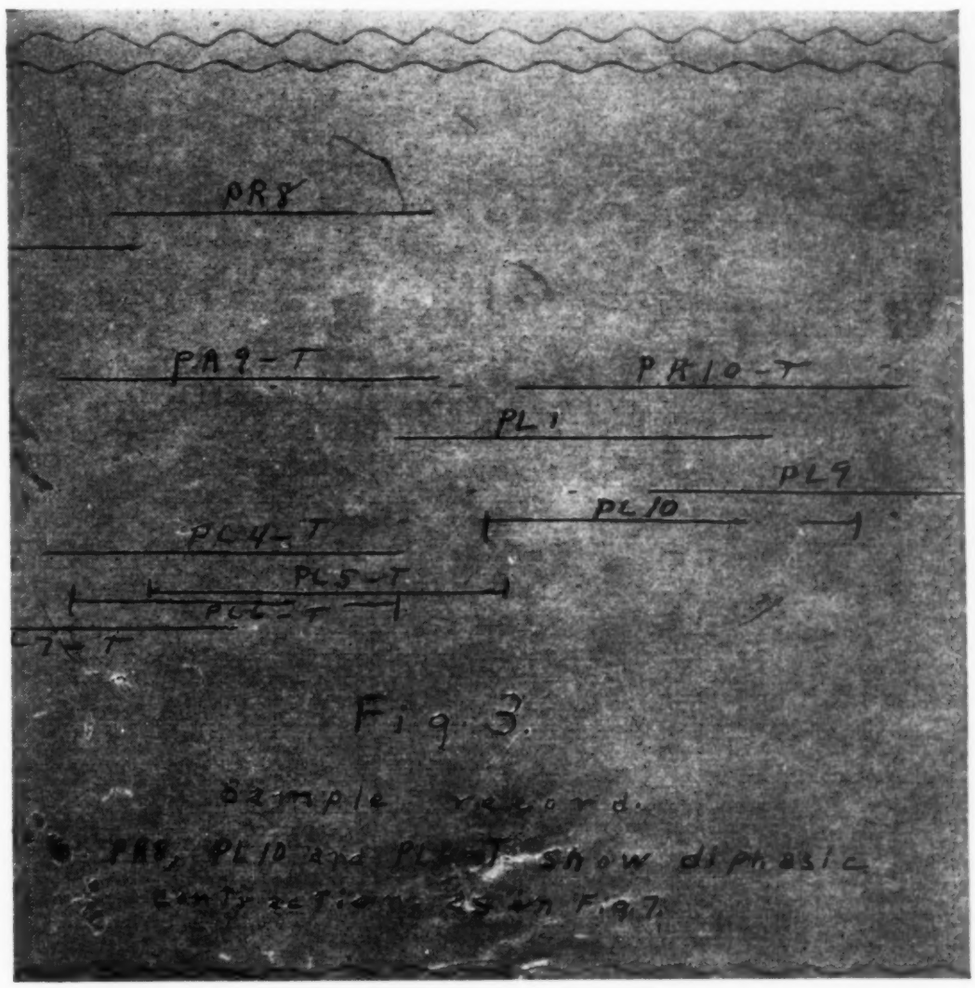

FIG. 3.

cient regularity, is the construction of a 'sigma rule' with which lengths of line can be read off directly in sigma. In the present model each 1/1000 second is represented by a line 1.26 mm. in length.

Calibration.—In determining the limits of accuracy of this type of instrument, 2 sources of variability are to be considered. They are fluctuations in the speed of the recording

drum and variations in the latent period of the marker. These factors were checked in the following way. Two contact points were attached to the chronograph in such a manner that the drum pressed them together momentarily once during each revolution. By including an electric impulse counter in the circuit thus established, it was possible to count the number of revolutions with absolute accuracy. The time of 100 revolutions was determined by means of a 1/10 second stop watch. The results of 50 measures of 100 revolutions each, extending over a period of 2 months, showed a maximum variation for the entire period of .2 second per 100 revolutions, or .002 second per revolution, or a maximum error of .0002 second for any one measure. The variabilities of the recording stylus were checked by placing it directly in circuit with the electric contact described above, so that it was actuated at a fixed point of each revolution. The stylus was mechanically fed across the paper by means of a second pulley

FIG. 4.

as already described. Figure 4 shows a typical record. The maximum difference in establishment of the contacts is not greater than .5 mm.

Whitehorn, Lundholm and Gardner (**8**) in a recent study of the knee-jerk, using the action-current technique, obtained lines on film of an actual length of 4 mm. representing latent times of approximately 20 sigma. Dodge (**2**, **3**) obtained lines of similar length by his modified myograph technique. Despite the fact that the beginning and end of these reactions had to be determined on the basis of the inception of curves, they were able to read differences in time as low as 0.25 sigma. Both Travis and Young (**7**) and Dodge (**2**) have, however,

pointed out the dangers involved in attempting to read records of such short length, the beginning and end of which were not marked as definite points. It will be seen that a latency of 20 sigma would on the present instrument be represented by a line approximately 25 mm. in length, the beginning and end of which are marked by the beginning and end of a clear, straight line, thereby eliminating the above-mentioned sources of error.

This chronograph has been used to make over 10,000 measurements of knee-jerk latencies. Figure 3 shows a section cut from a typical record. Seventy-five to 100 individual records can be made on one strip. These records, as Kreuger (**5**) points out, are not messy to handle, are permanent, and the records appear with great clarity. Records can be made as fast as desired. Knee-jerk records, for instance, can be made at the rate of 10 per minute.

It should be added that if available, any good synchronous motor could be used as the driving unit, thus assuring an absolutely uniform speed and doing away with the necessity of frequently checking this factor.

This apparatus should find a wide field of usefulness among institutions needing a reliable chronograph but unable to provide the costly mechanical devices now available. Its advantages over the costly and tedious chronographs using film are obvious, since here 100 reaction times can be recorded on about one cent's worth of recording paper. It can also be made to serve a wide range of usefulness as an ordinary polygraph.

(Manuscript received March 27, 1933)

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CORRECTION

In the article Stimulus dispersions in the method of constant stimuli, this JOURNAL, 1932, 15, 284–297, L. L. Thurstone has "developed a procedure for obtaining an approximate value for the dispersion of each stimulus." He found "the average discrepancy between the experimental and calculated proportions is 0.0355 by Case V in which the stimulus dispersions are assumed to be equal, but it is reduced to an average discrepancy of 0.0249 by Case III in which allowance is made for differences in stimulus dispersion." From this we can conclude that Case III is the better of the two. The author further states: "A criterion for deciding whether the simpler form of Case V or the more laborious Case III should be used for an experiment that has been conducted by the constant method is to plot pairs of columns of x-values in Table II, (of the article in reference). If the best fitting straight lines in these plots have a slope nearly equal to unity, then Case V can be used advantageously. If not then a better agreement between experimental and calculated values will be obtained by Case III."

It should be noted that there is an error in the printed formulæ in this article. If the criterion mentioned is used, formulæ (15) and (16) on page 293, give negative results due to the negative signs found therein. Since this shows Case V of the Law of Comparative Judgment to produce the best results, there is conflict with the previous calculated results.

If formula (14) on page 293 be examined it will be noted in solving for x_{1k} that $-.707\ x_{2k}\ (\sigma_2 + \sigma_k)$ will become a positive quantity when transposed. When plotted this result becomes positive and substantiates the findings from the calculated proportions. In formulæ (15) and (16) therefore, the minus signs in both cases should be changed to plus signs.

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